





AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LVIII. For the Year 1768.

L O N D O N :

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M.DCC.LXIX.

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THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought adviseable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

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should judge most proper for publication in the future *Transactions*; which was accordingly done up in the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of nature or art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons, through whose hands they receive them, are to be considered in no other light, than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL
TRANSACTIONS.

Received January 24, 1768.

- I. *An Account of the Eruption of Mount Vesuvius, in 1767: In a Letter to the Earl of Morton, President of the Royal Society, from the Honourable William Hamilton, His Majesty's Envoy Extraordinary at Naples.*

Naples, December 29, 1767.

My Lord,

Read Feb. 11, 1768. **T**HE favourable reception, which my account of last year's eruption of Mount Vesuvius met with from your Lordship, the approbation which the Royal Society was pleased to shew, by having ordered the same to be printed in
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their Philosophical Transactions, and your Lordship's commands in your letter of the 3d instant, encourage me to trouble you with a plain narrative of what came immediately under my observation during the late violent eruption, which began October 19, 1767, and is reckoned to be the 27th since that, which, in the time of Titus, destroyed Herculaneum and Pompeii.

The eruption of 1766 continued in some degree till the 10th of December, about nine months in all, yet in that space of time the mountain did not cast up a third of the quantity of lava, which it disgorged in only seven days, the term of this last eruption. On the 15th of December, last year, within the ancient crater of Mount Vesuvius, and about twenty feet deep, there was a crust, which formed a plain, not unlike the solfaterra in miniature; in the midst of this plain was a little mountain, whose top did not rise so high as the rim of the ancient crater. I went into this plain, and up the little mountain, which was perforated, and served as the principal chimney to the volcano; when I threw down large stones, I could hear that they met with many obstructions in their way, and could count a hundred moderately before they reached the bottom.

Vesuvius was quiet till March 1767, when it began to throw up stones, from time to time; in April the throws were more frequent, and at night fire was visible on the top of the mountain; or, more properly speaking, the smoak, which hung over the crater, was tinged by the reflection of the fire within the volcano. These repeated throws of cinders, ashes, and pumice stones, encreased the little
 5 mountain

mountain so much, that in May its top was visible above the rim of the ancient crater. The 7th of August there issued a small stream of lava, from a breach in the side of this little mountain, which gradually filled the valley between it and the ancient crater; so that the 12th of September the lava overflowed the ancient crater, and took its course down the sides of the great mountain; by this time, the throws were much more frequent, and the red hot stones went so high as to take up ten seconds in their fall. Padre Torre, a great observer of Mount Vesuvius, says they went up above 1000 feet.

The 15th of October, the height of the little mountain (formed in about eight months) was measured by Don Andrea Pignonati, a very ingenious young man in his Sicilian majesty's service, who assured me that its height was 185 French feet.

From my villa, situated between Herculaneum and Pompeii, near the convent of the Calmaldolese (marked 7 in the inclosed Plan I.), I had watched the growing of this little mountain, and by taking drawings of it from time to time, I could perceive its increase most minutely; I make no doubt but that the whole of Mount Vesuvius has been formed in the same manner; and as these observations seem to me to account for the various irregular strata, which are met with in the neighbourhood of volcanos, I have ventured to enclose for your Lordship's inspection a copy of the abovementioned drawings.

The lava continued to run over the ancient crater in small streams, sometimes on one side, and some times on another, till the 18th of October, when I took particular notice that there was not the

least lava to be seen, owing, I imagine, to its being employed in forcing its way towards the place where it burst out the following day. As I had, contrary to the opinion of most people here, foretold the approaching eruption^a, and had observed a great fermentation in the mountain after the heavy rains, which fell the 13th and 14th of October, I was not surprised on the 19th following, at seven of the clock in the morning, to perceive from my villa every symptom of the eruption being just at hand. From the top of the little mountain issued a thick black smoak, so thick that it seemed to have difficulty in forcing its way out; cloud after cloud mounted with a hasty spiral motion, and every minute a volley of great stones were shot up to an immense height in the midst of these clouds; by degrees, the smoak took the exact shape of a huge pine tree, such as Pliny the younger described in his letter to Tacitus, where he gives an account of the fatal eruption in which his uncle perished^b. This column of black smoak,

^a This plainly appears from the following extract of a letter, from the same gentleman to the president, dated Naples, October 6, 1767. "Mount Vesuvius is preparing for another eruption, or rather a second part of the last, as it has never been quiet since the beginning of the year 1765. The lava already runs over the crater; and by the quantity of stones and ashes, the montagnola has almost filled the crater, and has risen at least 80 feet within these last three months."

^b These are his words. "Nubes (incertum procul intuentibus ex quo monte Vesuvium fuisse postea cognitum est) oriebatur, cujus similitudinem & formam, non alia magis arbor, quam pinus expresserit. Nam longissimo veluti trunco elata in altum, quibusdam ramis diffundebatur, credo quia recenti spiritu erecta, dein senescente eo destituta, aut etiam pondere suo victa, in latitudinem evanescebat: candida interdum, interdum fordida & maculosa, prout terram cineremve sustulerat." Plin. Lib. VI, Ep. 16.

after

after having mounted an extraordinary height, bent with the wind towards Caprea, and actually reached over that island, which is not less than 28 miles from Vesuvius.

I warned my family not to be alarmed, as I expected there would be an earthquake at the moment of the lava's bursting out; but before eight of the clock in the morning I perceived that the mountain had opened a mouth, without noise, about 100 yards lower than the ancient crater, on the side towards the Monte di Somma; and I plainly perceived, by a white smoak, which always accompanies the lava, that it had forced its way out: as soon as it had vent, the smoak no longer came out with that violence from the top. As I imagined that there would be no danger in approaching the mountain when the lava had vent, I went up immediately, accompanied by one peasant only. I passed the hermitage (3. in Plan I.), and proceeded as far as the spot marked (X), in the valley between the mountain of Somma and that of Vesuvius, which is called Atrio di Cavallo. I was making my observations upon the lava, which had already, from the spot (E) where it first broke out, reached the valley, when, on a sudden, about noon, I heard a violent noise within the mountain, and at the spot (C) about a quarter of a mile off the place where I stood, the mountain split; and, with much noise, from this new mouth a fountain of liquid fire shot up many feet high, and then like a torrent rolled on directly towards us. The earth shook at the same time, that a volley of pumice stones fell thick upon us; in an instant, clouds of black smoak and ashes caused almost a total darkness; the explo-

sions

sions from the top of the mountain were much louder than any thunder I ever heard, and the smell of the sulphur was very offensive. My guide alarmed took to his heels; and I must confess that I was not at my ease. I followed close, and we ran near three miles without stopping; as the earth continued to shake under our feet, I was apprehensive of the opening of a fresh mouth, which might have cut off our retreat. I also feared that the violent explosions would detach some of the rocks off the mountain of Somma, under which we were obliged to pass; besides, the pumice-stones, falling upon us like hail, were of such a size as to cause a disagreeable sensation upon the part where they fell. After having taken breath, as the earth still trembled greatly, I thought it most prudent to leave the mountain, and return to my villa, where I found my family in a great alarm at the continual and violent explosions of the volcano, which shook our house to its very foundation, the doors and windows swinging upon their hinges. About two of the clock in the afternoon another lava forced its way out of the same place from whence came the lava last year, at the spot marked B (in Plan II.), so that the conflagration was soon as great on this side of the mountain as on the other, which I had just left.

The noise and smell of sulphur encreasing, we removed from our villa to Naples; and I thought proper, as I passed by Portici, to inform the court of what I had seen; and humbly offered it as my opinion, that his Sicilian majesty should leave the neighbourhood of the threatening mountain. However, the court did not leave Portici till about twelve lock, when the lava had reached as far as
(4. in

(4. in Plan. I.) I observed, in my way to Naples, which was in less than two hours after I had left the mountain, that the lava had actually covered three miles of the very road through which we had retreated. It is astonishing that it should have run so fast; as I have since seen, that the river of lava, in the Atrio di Cavallo, was 60 and 70 feet deep, and in some places near two miles broad. When his Sicilian majesty quitted Portici, the noise was greatly increased, and the confusion of the air from the explosions was so violent, that, in the king's palace, doors and windows were forced open, and even one door there, which was locked, was nevertheless burst open. At Naples, the same night, many windows and doors flew open; in my house, which is not on the side of the town next Vesuvius, I tried the experiment of unbolting my windows, when they flew wide open upon every explosion of the mountain. Besides these explosions, which were very frequent, there was a continued subterraneous and violent rumbling noise, which lasted this night about five hours. I have imagined that this extraordinary noise might be owing to the lava in the bowels of the mountain having met with a depolition of rain water, and that the conflict between the fire and the water may, in some measure, account for so extraordinary a crackling and hissing noise. Padre Torre, who has wrote so much and so well upon the subject of Mount Vesuvius, is also of my opinion; and indeed it is natural to imagine, that there may be rain water lodged in many of the caverns of the mountain, as, in the great eruption of Mount Vesuvius in 1663, it is well attested, that several towns, among

among which Portici and Torre del Greco, were destroyed by a torrent of boiling water having burst out of the mountain with the lava, by which thousands of lives were lost. About four years ago, Mount Etna in Sicily threw up hot water also, during an eruption.

The confusion at Naples this night cannot be described; his Sicilian majesty's hasty retreat from Portici added to the alarm; all the churches were opened and filled, the streets were thronged with processions of saints; but I shall avoid entering upon a description of the various ceremonies that were performed in this capital, to quell the fury of the turbulent mountain.

Tuesday the 20th, it was impossible to judge of the situation of Vesuvius, on account of the smoke and ashes which covered it entirely, and spread over Naples also, the sun appearing as through a thick London fog, or a smoked glass; small ashes fell all this day at Naples. The lavas on both sides of the mountain ran violently; but there was little or no noise till about nine o'clock at night, when the same uncommon rumbling began again, accompanied with explosions as before, which lasted about four hours; it seemed as if the mountain would split in pieces; and, indeed, it opened this night almost from the spot E to C (in Plan I.). The inclosed plans were taken upon the spot at this time, when the lava's were at their height; and I do not think them exaggerated. The Parisian barometer was, at yesterday, at 27.9, and Fahrenheit's thermometer at 70 degrees; whereas, for some days preceding the eruption, it had been at 65 and 66. During the confusion



confusion of this night the prisoners in the publick jail attempted to escape, having wounded the jailer, but were prevented by the troops. The mob also set fire to the cardinal archbishop's gate, because he refused to bring out the relicks of Saint Januarius.

Wednesday 21st was more quiet than the preceding days, though the lavas ran briskly. Portici was once in some danger, had not the lava taken a different course, when it was only a mile and a half from it; towards night the lava slackened.

Thursday 22d, about ten of the clock in the morning, the same thundering noise began again, but with more violence than the preceding days; the oldest men declared they had never heard the like, and, indeed, it was very alarming; we were in expectation every moment of some dire calamity. The ashes, or rather small cinders, showered down so fast, that the people in the streets were obliged to use umbrellas, or flap their hats, these ashes being very offensive to the eyes. The tops of the houses, and the balconies, were covered above an inch thick with these cinders. Ships at sea, twenty leagues from Naples, were also covered with them, to the great astonishment of the sailors. In the midst of these horrors, the mob growing tumultuous and impatient, obliged the cardinal to bring out the head of Saint Januarius, and go with it in procession to the Ponte Maddalena, at the extremity of Naples, towards Capri; and it is well attested here, that the eruption ceased the moment the Saint came in sight of the mountain; it is true the noise ceased about that time, after having lasted five hours, and had done the preceding day.

Friday 23d, the lavas still ran, and the mountain continued to throw up quantities of stones from its crater; there was no noise heard at Naples this day, and but little ashes fell there.

Saturday 24th, the lava ceased running; the extent of the lava, from the spot C (Plan I.), where I saw it break out, to its extremity I', where it surrounded the chapel of Saint Vito, is above six miles. In the Atrio di Cavallo, and in a deep valley, that lies between Vesuvius (1.), and the hermitage (3.), the lava is in some places near two miles broad, and in most places from 60 to 70 feet deep; at (4.) the lava ran down a hollow way, called Fossa grande, made by the currents of rain water; it is not less than 200 feet deep, and 100 broad; yet the lava in one place has filled it up. I could not have believed that so great a quantity of matter could have been thrown out in so short a time, if I had not since examined the whole course of the lava myself. This great compact body will certainly retain some heat many months; at this time, much rain having fallen for some days past, the lava smokes, as if it ran afresh: and about ten days ago, when I was up the mountain with Lord Stormont, we thrust sticks into the crevices of the lava, which took fire immediately: But to proceed with my journal.

The 24th Vesuvius continued to throw up stones as on the preceding days; during the whole of this eruption it had differed in this circumstance from the eruption of 1766, when no stones were thrown out of the crater from the moment the lava ran freely.

Sunday 25th, small ashes fell all day at Naples; they issued from the crater of the volcano, and formed a vast

a vast column, as black as the mountain itself, so that the shadow of it was marked out on the surface of the sea; continual flashes of forked, or zig-zag lightning shot from this black column, the thunder of which was heard in the neighbourhood of the mountain, but not at Naples; there were no clouds in the sky at this time, except those of smoke issuing from the crater of Vesuvius. I was much pleased with this phenomenon, which I had not seen before in that perfection.

Monday 26th, the smoke continued, but not so thick, neither were there any flashes of the mountain lightning. As no lava has appeared after this column of black smoke, which must have been occasioned by some inward operation of fire, I am apt to think that the lava, which should naturally have followed this symptom, has broke its way into some deeper cavern, where it is silently brooding future mischief; and I shall be much mistaken if it does not break out a few months hence.

Tuesday 27th, no more black smoke, nor any signs of eruption.

Thus, My Lord, I have had the honor of giving your Lordship a faithful narrative of my observations during this eruption, which is universally allowed to have been the most violent of this century; and I shall be happy if it should meet with your approbation, and that of the Royal Society, if your Lordship should think it worthy of being communicated to so respectable a body.

I have just sent a present to the British Museum of a compleat collection of every sort of matter produced by Mount Vesuvius, which I have been col-

lecting with some pains for these three years past, and it will be a great satisfaction to me if, by the means of this collection, some of my countrymen, learned in natural history, may be enabled to make some useful discoveries relative to volcanos^c.

I have also accompanied that collection with a current of lava from Mount Vesuvius; it is painted with transparent colours, and, when lighted up with lamps behind it, gives a much better idea of Vesuvius, than is possible to be given by any other sort of painting.

I have the honor to be,

My Lord,

Your Lordship's

most obedient,

and most humble servant,

William Hamilton.

^c " I am well convinced, by this collection, that many variegated marbles, and many precious stones, are the produce of volcanos; and that there have been volcanos in many parts of the world, where at present there are no traces of them visible." This is taken from a prior letter of Mr. Hamilton, to the President, dated April 7, 1767.

P L A T E I.

- A. Crater of Mount Vesuvius.
 - B. Mouth from whence came the lava of 1766 ;
and which opened afresh, October 19, 1767,
and produced the conflagration represented
in Plan II.
 - C. The mouth which opened at 12 o'clock, Oc-
tober 19, 1767, whilst I was at the spot
marked x ; from thence came all the lava
represented in Plan I.
 - D. The lava.
 - E. Mouth from whence the lava flowed at eight
o'clock, October 19, when the eruption be-
gan first.
 - F. Chapel of Saint Vito surrounded with lava.
1. Vesuvius.
 2. Mountain of Somma.
 3. Hermitage, between which and Vesuvius there
is a deep valley two miles broad.
 4. The Fossa Grande.
 5. His Sicilian Majesty's Palace at Portici.
 6. Church of Pugliano.
 7. Calmaldolese Convent, near which is my Villa.
 8. Saint Torio.
 9. Barra.
 10. Spot, under which lies Herculaneum.

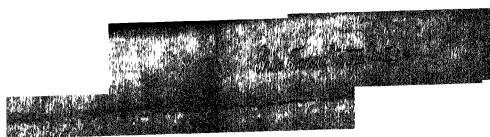
PLATE II.

- A. Crater of Vesuvius.
- B. Mouth, from whence came the lava of 1766, and which opened afresh at two o'clock, October 19, 1767, and caused the conflagration on this side of the mountain.
- C. Mouth which opened at 12 o'clock, October 19, 1767, whilst I was at the spot X, and which produced all the lava represented in Plan I.
- D. Rivulets of lava, which flowed from the crater, and united with the great river E.
- F. Extremities of the lava, about five miles from B.

- 1. Mountain of Somma.
- 2. Mount Vesuvius.
- 3. Montagna di Trecafe.
- 4. Trecafe.
- 5. Oratorio di Bosco.
- 6. Ottaviano.

PLATE III.

View of the gradual increase of the lava mountain within the ancient crater, and of the present state of Mount Vesuvius.



Received November 26, 1767.

II. *Extract of a Letter, dated Vienna April 4, 1767, from Father Joseph Liefganig, Jesuit, to Dr. Bevis, F. R. S. containing a short Account of the Measurement of Three Degrees of Latitude under the Meridian of Vienna.*

Read Feb. 18, 1767. **P**O S T paucas septimanas prelo committam opusculum meum de dimensione trium fere graduum meridiani nostri, Augustæ nostræ jussu a me suscepta. Continetur arcus totus quem dimensus sum, inter pagum Sobieschiz (Brunnâ, Moraviæ urbe, $3\frac{1}{2}$ borealiorem) ac inter Varasdinum Croatiae. Arcum terrestrem definivi 22 triangulis sat magnis, si ea demam, quæ duas bases (quarum quævis 6000 hexapedarum Parisinarum superat) cum triangulorum serie connectunt. Arcus cœlestis amplitudinem determinavi per observationes plurium fixarum, quas feci sectore decem pedum, non in extremis duntaxat arcus mei, locis Sobieschizii videlicet et Varasdini, verum etiam Brunnæ, Viennæ, et Græcii, Styriæ metropoli: quo magnitudinem unius gradus, tum ex arcu toto, tum ex interjectis partialibus arcubus deducere liceret. Nec imprudenter id a me actum esse eventus docuit: nam gradum inter Viennam et Græcium interceptum 186 hexapedas Parisinas minorem reperi eo, qui Viennâ Brunnam, Boream versus porrigitur, Varasdinensem fere 300 hexapedis Brunnensi majorem. Insignem hanc differentiam attractioni ingentium Styriæ superioris ac inferioris montium, quibus Græcium subjicitur,tribuendam esse ostendam. Adjecta pagina compendia totius laboris ac opusculi mei summam exhibet.

cus Meridiani Cœlestis.

	Dracon	γ Dracon.	α Cœni.	α Cœni.	Capræ.	β Aurigæ.	Medium.
Inter Sobieſchiz, et Brunnam et Viennam et Græcium et Varadin.	0 3 35,8 1 2 29,6 2 10 54,5	0 3 35,8 1 2 29,6 2 10 54,5	0 3 35,8 1 2 29,6 2 10 54,5	0 3 35,8 1 2 29,6 2 10 54,5	0 3 35,8 1 2 29,6 2 10 54,5	0 3 35,8 1 2 29,6 2 10 54,5	0 3 35,8 1 2 29,6 2 10 54,5
Inter Brunnam et Viennam et Græcium et Varadin.	0 58 53,5 1 2 18,5 2 7 18,5	0 58 53,6 1 2 18,5 2 7 18,5	0 58 53,6 1 2 18,5 2 7 18,5	0 58 53,6 1 2 18,5 2 7 18,5	0 58 53,6 1 2 18,5 2 7 18,5	0 58 53,6 1 2 18,5 2 7 18,5	0 58 53,6 1 2 18,5 2 7 18,5
Inter Viennam et Græcium et Varadin.	0 8 24,9 1 8 24,9 2 8 24,9	0 8 24,7 1 8 24,7 2 8 24,7	0 8 24,7 1 8 24,7 2 8 24,7	0 8 24,7 1 8 24,7 2 8 24,7	0 8 24,7 1 8 24,7 2 8 24,7	0 8 24,7 1 8 24,7 2 8 24,7	0 8 24,7 1 8 24,7 2 8 24,7
Inter Græcium et Varadin.	0 45 50,5 1 45 50,5 2 45 50,5	0 45 50,5 1 45 50,5 2 45 50,5	0 45 50,5 1 45 50,5 2 45 50,5	0 45 50,5 1 45 50,5 2 45 50,5	0 45 50,5 1 45 50,5 2 45 50,5	0 45 50,5 1 45 50,5 2 45 50,5	0 45 50,5 1 45 50,5 2 45 50,5
	Amplit. circuli cœlestis.	Diffantia parallelorum.	Ex ferie triangulorum.	Magnitudo unius Hex. Vienn.	Hex. Paril.	Gradus. Red. ad libell. maris.	
Inter Sobieſchiz et Varadin.	2 56 45,8 1 17 280,8 2 17 280,8	17 280,8 17 280,8 17 280,8		586 57,5 570 79,8 570 7,4	570 7,4 570 7,4 570 7,4	570 7,4 570 7,4 570 7,4	
Inter Sobieſchiz et Viennam	1 2 29,0 1 610 94,0 2 610 94,0	610 94,0 610 94,0 610 94,0	I. II. III.	586 5,9 586 2,7 586 6,9	570 8,0 570 8,9 570 8,3	570 8,0 570 8,9 570 8,3	
Inter Brunnam et Viennam	0 58 53,5 1 57 588,5 2 57 588,5	57 588,5 57 588,5 57 588,5		586 72,3 586 5,4 584 67,2	570 94,2 570 87,5 568 94,6	570 94,2 570 87,5 568 94,6	
Inter Viennam et Græcium vel	1 8 24,8 1 8 24,8 2 8 24,8	666 81,8 666 81,8 666 81,8	Medium	584 67,2 584 67,2 584 67,2	569 08,5 569 08,5 569 08,5	570 8,1 568 92,2 569 06,1	
Inter Græcium et Varadin.	0 45 49,9 1 45 49,9 2 45 49,9	450 32,3 450 32,3 450 32,3	Medium	584 74,3 589 53,5 589 53,5	569 01,5 573 67,9 573 67,9	568 99,1 573 65,5 573 65,5	

Received November 16, 1767.

III. *An Essay on the Force of Percussion,*
by William Richardson, M. D. commu-
nicated by William Heberden, M. D.
F. R. S.

Read Feb. 18, 1768. **W**HEN we consider the extraor-
dinary advancement in natural
philosophy, from the surprising discoveries of the
great Sir Isaac Newton, and other ingenious men,
who have followed his example; it may afford mat-
ter of the greatest wonder, to find the most acute
philosophers still contending, whether the force of
percussion be in proportion to the velocity of bodies
in motion, or the squares of those velocities.

Sentiments so opposite in their nature, and so
strongly supported by their respective advocates, may
well make a cautious person suspect, that nothing is
to be discovered with certainty in the operations of
nature. In which opinion he may be the more con-
firmed from this consideration, that the present
dispute is not about objects far removed from our
observation, but such actions of bodies as do con-
stantly occur to our senses, and which without dif-
ficulty may be reduced to experiment.

What then can be the reason, after such variety
of experiments have been made, why this matter is
not, before this time, brought to a decision? The

fault cannot be in the experiments themselves, the chief of which have been often repeated, and that in the most accurate manner : it must, therefore, be in the use which has been made of them, in the groundless inferences which have been drawn from them.

Those who have wrote best on this subject (whether in support of the velocities, or the squares of velocities) seem to me to have inferred more from the principle they maintain than what they bring sufficient arguments to justify ; by which means they blend truth with error, and the more they endeavour to illustrate their respective doctrines, the more they render them perplexed and confused. For the truth of what I have advanced, I appeal to the two following instances.

Those who maintain that the force of percussion is as the velocity of the striking bodies, when they account for the impressions made in soft bodies (which are found, by experiment, to be as the squares of the velocities), inform us, that the time ought to be taken into the account ; which being as the velocity of the impinging body, the impression will of course be as the time into the velocity, or (which is the same thing) as the square of the velocity.

But this, in my mind, is to assert more than what can be clearly demonstrated : for as action and reaction are equal, the more forcibly one body acts upon another, the greater must be the resistance it meets with ; and whatever be the time of its acting, supposing its force to be given, the effect produced must still remain the same.

But I will not be so far from saying, that the effect produced by a given force, is as the time of its acting, and as the velocity of the body.

To which I must further add, that some of the most learned and zealous advocates for time being taken into the account, have not agreed among themselves whether it be in a direct or reciprocal proportion of the velocity^a.

Those who, on the contrary, insist that the force of percussion is in proportion to the squares of the velocity, finding from experiment that in soft bodies the velocity after percussion falls short of this estimate, would make us believe, that in compressing the parts of those bodies, a certain degree of force must necessarily be lost, which, being added to what remains after percussion, will sufficiently confirm the truth of their doctrine.

To this I reply, that the parts of soft bodies are, indeed, removed out of their places by the stroke, and that some motion is lost in the impinging body, being communicated to the parts of the soft body it strikes upon; but these parts cannot lose their motion any other way, than by communicating it to other parts, or by the force accruing to the whole body.

How then are these different effects to be accounted for, and in what manner are they to be deduced from the same cause? This diversity of appearances, I have for some time suspected, might proceed from the nature of cohesion: that while the force of percussion produced an effect on the whole mass of matter which receives the stroke, in proportion to the velocity of the impinging body; it might, at the

^a Dr. Pemberton, *Philosophical Transactions*, N^o 371. p. 57.
Dr. Clarke, *Philosophical Transactions*, N^o 401. p. 382.

same time, in separating the cohering parts from each other, produce an effect in proportion to the square of the velocity.

Into which way of thinking I was first led from the following observations ; that a chord, which would bear a very strong pull, might easily be broken by giving it a sudden jerk ; as also that the weight of a hammer did not contribute so much in driving a nail, as the quickness of the motion given it by the driver.

In order to make a further discovery, whether or no this my supposition was really founded in nature ; I determined first to make experiments on such soft bodies as have a considerable degree of cohesion ; and then to try those bodies, when dried and reduced to powder, and by that means deprived of their cohesion ; which experiments, when compared with each other, would, I flattered myself, give me an insight into this intricate affair, and at the same time disclose that beautiful simplicity, which nature observes in all her operations.

My apparatus for making the experiments consists of four balls exactly spherical, two iron branches, and a small lead cistern.

The balls are each of them two inches in diameter ; two are of brass, and two of box-wood ; one of each sort is solid, and the other hollow ; that which is hollow is only half the weight of the solid one, and may be opened by means of a screw in the middle.

The iron branches are to give the balls their proper directions ; they have each of them a small brass pulley in the fore part, and in the hind part a kind of hook,

hook, which fastens them to staples at different heights; one of the branches is two inches long, and the other four inches, exclusive of pullies; by which means the balls when let fall are directed to different parts of the surface they strike upon.

The lead cistern is of an oblong form, that the matter therein contained may, at the same time, receive two distinct impressions; either when balls of different weight are let fall, or the same ball is let fall from different heights; its length is six inches, its breadth four inches, and its depth two inches.

The matter I have found best suited to the purpose is stiff clay, tempered in such a manner as to be smooth and uniform, with the same reduced to powder, after having been baked in an oven, as also after having (by a still stronger heat) been converted into brick.

The cistern is by turns filled with these different materials, which are to be closely and uniformly pressed down, so as to leave the surface quite level: in effecting which, great caution is required, more particularly in regard to the powders; as they will not distinctly retain the impressions, unless they have some small degree of moisture, or be very closely pressed down; in both which cases they acquire such a degree of cohesion, as of course must render the experiments more or less imperfect.

Things being thus prepared, in order to try the necessary experiments, I fixt the staples, and by their means the branches, at the following heights, viz. two feet, four feet, and eight feet; the result of which experiments was as follows.

When

When the brass balls, in weight to each other as two to one, were let fall on tempered clay, from four feet and eight feet respectively, the impressions made were on various trials found to be equal.

When the wood balls, being to each other as two to one, were let fall from the same heights, on dried clay pulverised, that from four feet generally made the deeper impression.

When the wood balls were let fall from the same heights on brick-dust, that from four feet constantly made the deeper impression.

When the lighter brass ball was let fall on tempered clay, from two feet and eight feet, the impressions were to each other as one to four.

When the lighter wood ball was let fall on dried clay pulverised, from the same heights, the impressions were (so far as the eye could judge) nearly in the proportion of one to three.

When the same ball was let fall on brick-dust, from the like heights, the impressions were not much short of the proportion of one to two.

From these experiments it plainly appears: First, That the impressions made in soft clay are in proportion to the heights, from whence the balls are let fall, consequently as the squares of their respective velocities. Secondly, That the impressions, in pulverised clay, recede considerably from that proportion, being as it were in the medium between the squares of the velocities and the velocities themselves. Thirdly, That the impressions in brick-dust are nearly in a subduplicate proportion of the heights from whence the balls are let fall, consequently vary but

but little from the proportion of the velocities acquired.

Whence I should apprehend it clearly follows; that the impressions made in soft bodies, by hard ones striking upon them, do vary from each other, according to the degree of cohesion in the respective soft bodies; and that the impressions would be in exact proportion of the velocities, could their form be perfectly retained by bodies quite void of cohesion.

Nothing, however, being more evident to me than that actions ought to be measured by their effects, and at the same time fully depending on the accuracy of the experiments, I am determined to rest this important point entirely upon them. Shall not, therefore, attempt any illustration in the mathematical way, lest, by too far indulging a favourite opinion, I should bewilder myself in intricate calculations. Much less shall I endeavour to establish my doctrine on metaphysical principles, which seem to me in themselves too obscure, to throw any clear light on subjects of this nature.

Received November 26, 1767.

IV. *An Essay on the Connexion between the Parallaxes of the Sun and Moon; their Densities; and their disturbing Forces on the Ocean.* By Patrick Murdoch, D. D. F. R. S.

Read Feb. 18, 1768. **I**N a letter to Mr. Reid, formerly presented to the Hon. Society, and printed in Vol. LIV. Part II. of the Transactions, mention was made of a rule which I had used for computing the sun's parallax; but as that rule, though it gave a solution near the truth, was in part founded on authority (which, however respectable, ought to be cautiously admitted in such enquiries), I have considered the subject anew, upon such principles alone, as the established theory and the best observations furnished me. And the result is now humbly submitted to the Society.

I. The length of a second-pendulum at the equator, and on a level with the sea, being 36 inches $7\frac{1}{8}$ lines, Paris measure, according to the accurate observations of M. de la Condamine*, that length

* Journal du Voyage, &c. p. 163. In the copy of this paper, which was read in the Society, the length of a second-pendulum, at the equator, had been computed from its length at London: but here it is taken from the immediate observations of a very able astronomer. Some other small alterations have been made; and the examples placed in a better order.

(properly

(properly corrected) will, by the reasoning in the abovementioned letter, give the distance of a moon circulating round an unmoveable earth, equal to 59.95792 semidiameters of the equator. For the logarithm of this number, which is 1.7778438, write L .

Let L be the logarithm of some greater mean distance, inferred from observations of the moon's parallax; and if r be the natural number to the logarithm $3 \times \overline{L - l}$, and M be taken equal to $\frac{1}{r-1}$, the mass of the earth will be to that of the moon, as M to 1.

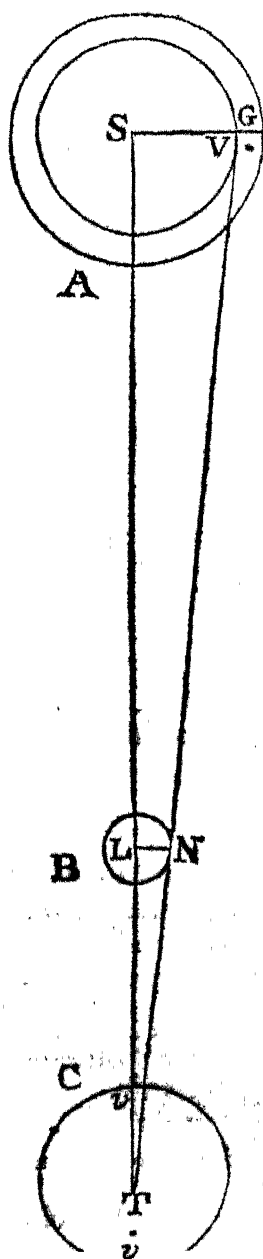
Conversely, If M be any how determined, its equal $\frac{1}{r-1}$, and r , with its logarithm $3 \times \overline{L - l}$ are known; $\frac{1}{3}$ of which is $\overline{L - l}$ to be added to L . For instance, if, with Sir Isaac Newton, we put $M = 39.788$, the distance will be 60.4557, the logarithm L being 1.7814372.

II. If, for each of these three, the moon's mass, her accelerative force on the earth, and her distance from the earth's centre, we write ($\phi =$) 1: the accelerative force of the earth on the moon will be represented by M , the mass just now computed. And if F is the sun's accelerative force on the earth, x his distance in semidiameters of the lunar orbit, Q the ratio of a sidereal year to a periodic month; we have (by Cor. 2. Prop. 4. Princip. I.) $\frac{F}{x} = \frac{M}{Q}$; a given ratio in given terms.

III. The terms F , x , therefore, must involve a common factor; by which being divided, the quote may be $\frac{M}{Q^2}$. And this might be obtained innumerable ways, were we to consider the ratio $\frac{F}{x}$ merely as an abstract quantity, altogether unrestricted: it were only putting $M^n \times Q^p = F$. And $\frac{Q^{2+p}}{M^{1-n}} = x$, or $M^{1-n} Q^p = F$, and $\frac{Q^{2+p}}{M^n} = x$: so as the sum of the *indices* of M should be unity, and the difference of those of Q should be 2.

But though the quantities F , x , are as yet unknown, they are not for that indeterminate and variable, as such a liberty of substitution would import: and all substitutions which imply the contrary, all *indices* which the theory disowns, or which are inconsistent with observation, are to be rejected. In a word, the *indices*, n , p , ought each of them to be *unique*, and determinate (*sine compare*), * as the quantities F and x are in nature. I take, therefore, $p = 1$, and $n = \frac{1}{2}$; that is, $F = M^{\frac{1}{2}} \times Q$, $x = \frac{Q}{M^{\frac{1}{2}}}$. See the examples in the table subjoined, upon different suppositions of the moon's distance.

* See Neut. Arith. Universal. in the Schol. to Prop. xxiv. The *maxima* and *minima* of variable quantities; the coordinates belonging to a double point, or to a point of reflexion, or contrary flexure, rays of curvature, limits of ratios, &c. All these are examples of the *unique*; that is, of quantities in a state that is distinguished from and exclusive of all others.



IV. The accelerative forces of two spherical bodies, A, B, upon a third C, are directly as their masses, and inversely as the squares of their central distances ST, LT, (or of x and d): which may be thus expressed, $\frac{F}{\phi} = \frac{A}{B} \times \frac{d^2}{x^2}$.

But the masses, A, B, being as their respective densities (which call s, m), and the cubes of (R, r) , the semidiameters of the spheres conjunctly; if we write for A

its equal, we have $\frac{F}{\phi} = \frac{s}{m} \times \frac{R}{r^2} \times \frac{d^2}{x^2}$.

Let the mass C be to B, as M to 1; and (f) its force on B, will be ϕM , or $\phi = \frac{f}{M}$, which gives $\frac{MF}{f} = \frac{s}{m} \times \frac{x}{d}$, and $\frac{F}{f} = \frac{s}{m} \times \frac{x}{d}$: supposing the apparent semidiameters of A and B to subtend the same angle at the centre of C, and thence R to be to r , as x to d .

V. But if SG, the semidiameter of A, be to the supposed semidiameter SV, as g to 1, then,

the density s remaining, the accelerative force of A (proportional to its magnitude) will be increased in the triplicate of that ratio, that is, we now have

$$\frac{F}{f} = \frac{q^3 s}{M m} \times x, \text{ putting } d = 1.$$

And the three bodies, A, B, C, representing the sun, moon, and earth; likewise Q being the ratio of the periods of the earth and moon, it is $\frac{F}{f} = \frac{x}{Q^2}$ (by the corollary quoted in Art. II). Whence $\frac{s}{m} = \frac{M}{Q^2 \times Q^2}$.

VI. This accelerative force of A remaining, imagine the semidiameter S G to be reduced to its former magnitude S V; and the density of A will, at the same time, be increased to $s' = q^3 \times s$, and $\frac{s'}{m} = \frac{M}{Q^2}$.

In which case, namely, when the apparent semidiameters of A and B (the sun and moon) are equal, their powers to raise a tide at v , a vertex of C, will be as the densities s' , m * : that is, as M the ratio of the earth's mass to the moon's, and Q^2 the duplicate ratio of the year and month.

Or thus : The distances of the bodies, A, B, from the third C, being very great, their powers to raise a tide at v , or their disturbing forces on the ocean, will be directly as their accelerative forces at the centre of C, and inversely as their distances from it: that is, writing a , b , for the disturbing forces respectively; and for the sun's distance in semidiameters of C, the Letter x , it will be $a : b :: \frac{F}{x} : \frac{f}{d}$.

* See the last page of Dr. Sanderfon's Fluxions: or the late ingenious T. Simpson's Miscellaneous Tracts, p. 13.

For, by the general law, a is to F , as the difference of the squares of z and $z-1$ is to the square of $z-1$; that is, as $2z-1$ to z^2-2z+1 . And the same way b is to ϕ , as $2d-1$ to d^2-2d+1 ; whence, halving the antecedents, and retaining only z^2 , d^2 , in the consequents, we have the ratio of a to b , as above.

If the disturbing force of B is exerted at v^1 , the opposite vertex of C , b will now be to ϕ , as $2d+1$ to d^2+2d+1 ; and, in strictness, we ought to take a mean value of b : but this may be neglected as inconsiderable.

Lastly, let the sun's distance be again expressed in semidiameters of the lunar orbit; that is, if for z we write dx , and unity for ϕ , we have $a:b::\frac{F}{dx}:\frac{1}{d}$, or as M to Q^1 , as before.

VII. In Art. V. it was found that m denoting the density of the moon, and s that of the sun, q^3 being triple the ratio of the sun's mean semidiameter to the moon's *, then will $\frac{m}{s} = \frac{Q^2 q^3}{M}$. Whence it will easily follow, that the density of the earth is to that of the sun, as $Q^2 \times S^3$ to P^3 ; P being the moon's horizontal parallax, and S the sun's apparent semidiameter.

* In the *Principia*, the semidiameters are $16'. 6''$, and $15'. 38\frac{1}{2}''$; giving 0.0379755 for the logarithm of q^3 . Others take a few seconds from each, which does not much alter the value of q^3 .

VIII. I have applied these rules (as in the following Table) to the principal hypothesis of the moon's mean distance.

- 1°. Supposing it of 60.24 (from Dr. M. Stewart.)
 2°. ————— 60.4 (Cor. 7. Prop. 37. Princip. III.)
 3°. ————— 60.455 (Neut. M being 39.788.)
 4°. ————— 60.493 { (As by Mr. Short's calculations
 from the transit of ♄.)

The Moon's Mean Distance, being in Semidiameters of the Equator.

		60.24	60.4	60.455	60.493
I. Parall.	☽	57.4,17	56.55 $\frac{1}{4}$	56.52	56.49,88
II. Mafs	☽	70.4225	44.823	39.788	36.9908
III. ☉ diff. to ☽		284.723	356.885	378.293	392.854
IV. Parall.	☉	12 $\frac{1}{37}$	9,58	9 $\frac{1}{2}$	8.69
V. Denf.	☉	2,77 : 1	4,35 : 1	4,9 : 1	5,273 : 1
VI. Denf.	☽	1,449 : 1	0,9295 : 1	0,827 : 1	0,7707 : 1
VII. Denf.	☉	4,0128 : 1	4,045 : 1	4,0559 : 1	4,06375 : 1
VIII. Fides	☉	2,5379 : 1	3,9873 : 1	4,5544 : 1	4,8316 : 1
IX. F. ☉ on ☽					
IX. F. ☽ on ☉		1,593 : 1	1,9968 : 1	2,1194 : 1	2,1981 : 1

REMARKS.

1. If it should be thought that the reasoning in Art. III. rests too much upon a metaphysical principle of Leibnitz, and requires, if not an apology, at least a more formal proof: the ground of such reasoning, and its extensive use, may be more particularly explained on some other occasion. Suffice it at present to add to the note on that article,

That as the given factors (Q and Q^3) in F and x , may be joined with M^n , or with M^{1-n} *indifferently*, the case is similar to that of an equal chance at play, for the stakes n and $1-n$, where the just expectation of each gamester is $\frac{1}{2}$, whatever be the value of n . The fifth and sixth Propositions of Element I. scarce needed any other demonstration than, that it is manifestly impossible to assign any *reason* of inequality, of the angles in one of these theorems, and of the sides in the other. In the following Proposition, where it is proved, that two lines being extended from the extremities of a right line, AB , to a point C , there is no other point on the same side of AB , to which lines from A and B equal to the former can be drawn: he who holds the contrary is supposed to fix upon that other point D ; but why D ? rather than d , d' , d'' , &c. he is silent: and, therefore, I conclude, there is no such point different from C . And the like may be said of some other simple theorems, that are commonly demonstrated by shewing the absurdity of asserting their contraries.

2. The

2. The title of this paper renders it almost needless to remind the reader, that the moon's parallax is not here proposed as the properest *medium* for determining that of the sun. Our *data* are still too uncertain for that purpose, scarce one of them having been determined to an unexceptionable precision; and the numbers in the table shew how much a small difference in the moon's distance must affect the several conclusions. It may be of use, however, to know in what manner those conclusions, as well as the quantities from which they derive*, stand related to one another. For if, hereafter, the necessary *data* should be more exactly known, the calculus may be repeated; and if the transit of Venus, which is to happen in 1769, should confirm Mr. Short's calculations from that of 1759, we may thence conclude the true mean distance of the moon, better than in any other way.

3. In the mean time, if any person should have the curiosity to examine the numbers of the table, he will please to take notice:

That, as no two measurements, nor any two lengths of a second-pendulum hitherto observed, make the earth of the same spheroid figure, I have retained for the ratio of its greatest and least diameters, that of 231 to 230; answering to the hypothesis of its uniform density: and have thence made a degree of the equator equal to 57200 French

* The connexions of F , x , Q , are manifest; and the relation of M to Q is easily deduced from Prop. 59. Princip. Book I.

† This was computed upon the supposition, that a degree of the meridian at lat. 49° is 57183 toises: but if that degree is, by The

The measure of a second-pendulum, at the equator, I had from M. de la Condamine, as was said before : it was corrected for the centrifugal force, and the resistance of the air ; and the moon's distance, as revolving round the earth at rest, was corrected for the sun's disturbing force : which is done either by diminishing that distance in the subtriplicate ratio of 178725 to 177725 ; or by diminishing the length of the pendulum in the simple ratio of these numbers. In this last correction I was favoured with the advice and assistance of the Rev. Mr. Price, and of the Astronomer Royal, FF. R. S.

a correction of M. Picart's operations, found to be only 57075 toises ; the diameter of the equator here used ought to be diminished by about 18 10000th parts.

Received December 20, 1767.

V. *Observations on the Bones, commonly supposed to be Elephants Bones, which have been found near the River Ohio in America: By William Hunter, M.D. F.R.S.*

Read February 25, 1768. **N**ATURALISTS, even those of our own times, have entertained very different opinions concerning fossil ivory, and the large teeth and bones, which have been dug up in great numbers in various parts of the world.

At first, some thought them animal substances, and others mineral. When only a certain number of observations had been collected, these substances were determined to be mineral: but, the subject having been more carefully examined, they were found certainly to be parts of animals.

After this point was settled, a dispute arose, to what animal they belonged. The more general opinion was, that they were bones of the elephant; and the great similitude of the fossil tusks to the real elephants teeth gave this opinion considerable credit.

It was liable however to great objections: the bones were observed to be larger than those of the elephant; and it was thought strange that elephants should have been formerly so numerous in western countries, where they are no longer natives, and in cold countries, Siberia particularly, where they cannot now live.

We had information from Muscovy, that the inhabitants of Siberia believed them to be the bones of the mammoth, an animal of which they told and believed strange stories. But modern philosophers have held the mammoth to be as fabulous as the centaur.

Of late years the same sort of tusks and teeth, with some other large bones, have been found, in considerable numbers, near the banks of the Ohio, in North America. The French Academicians became possessed of some specimens of them; and having compared them with the bones of real elephants, and with those which had been brought to France from Siberia, and with similar bones found in various other parts, determined, with an appearance of probability on their side, that they were elephants bones.

Monsieur Buffon gives us the following account of this decision *: “ All this put together, leaves us
 “ no longer any room to doubt, that those tusks
 “ (*defenses*), and those large bones (*ossemens*), are
 “ truly the tusks and bones of the elephant.
 “ M. Sloane had said this, but had not proved it.
 “ M. Gmelin has likewise said so, and more positively ;

* “ Tous cela réuni, fait que nous ne doutons plus que ces
 “ défenses & ces ossemens ne soient en effet des défenses & des
 “ ossemens d'éléphant. M. Sloane l'avoit dit, mais ne l'avoit
 “ pas prouvé. M. Gmelin l'a dit encore plus affirmativement, &
 “ il nous a donné sur cela des faits curieux ; mais M. Daubenton
 “ nous paroît être le premier, qui ait mis la chose hors de doute,
 “ par des mesures précises, des comparaisons exactes, & des
 “ raisons fondées sur les grandes connoissances qu'il s'est ac-
 “ quisées dans la science de l'anatomie comparée.” Hist. Na-
 turelle, Tom. XI. p. 87.

“ and he has given us some curious facts concern-
 “ ing this question ; — — — but M. Daubenton
 “ appears to us to be the first who has put the mat-
 “ ter beyond doubt, by accurate measures, by exact
 “ comparisons, and by reasons founded upon the
 “ great knowledge which he has acquired in the
 “ science of comparative anatomy.”

From the first time that I learned this part of natural knowledge, it appeared to me to be very curious and interesting ; inasmuch as it seemed to concur with many other phenomena, in proving, that in former times some astonishing change must have happened to this terraqueous globe ; that the highest mountains, in most countries now known, must have lain for many ages in the bottom of the sea ; and that this earth must have been so changed with respect to climates, that countries, which are now intensely cold, must have been formerly inhabited by animals which are now confined to the warm climates.

Some time in the last spring, having been informed that a considerable quantity of elephants teeth had been brought to the Tower, from America ; and being desirous of procuring some information concerning them, I waited upon Mr. Bodington, to know the particulars, and to beg leave to examine them. He obligingly gave me a verbal account of their having been brought from the banks of the Ohio ; and on the following day sent me one tusk, and one grinder, as specimens for my examination. The tusk, indeed, seemed so like that of an elephant, that there appeared no room for doubt. I shewed it to my brother, and he thought so too : but, being particularly conversant with comparative anatomy, at the
 first

first sight he told me that the grinder was certainly not an elephant's. From the form of the knobs on the body of the grinder, and from the disposition of the enamel, which makes a crust on the outside only of the tooth, as in a human grinder, he was convinced that the animal was either carnivorous, or of a mixed kind. This made me think that the tusk itself was not a real elephant's tooth: for Mr. Bodington had told me, that there were many grinders, as well as tusks, and that they were all similar to those specimens which he had sent to me. And some time after, when I went to the Tower, and examined the whole collection which had been sent over from the Ohio, I saw that the grinders were all of the same kind. I examined two elephants jaws in my brother's collection: I examined the tusks and grinders of the Queen's two elephants: and I examined a great number of African elephants teeth at a warehouse.

From all these observations I was convinced that the grinder tooth, brought from the Ohio, was not that of an elephant; but of some carnivorous animal, larger than an ordinary elephant: and I could not doubt that the tusk belonged to the same animal. The only difference that I could observe between it and a real elephant's tusk was, that it was more twisted, or had more of the spiral curve, than any of the elephants teeth which I had seen.

Some time after this, Dr. Franklin received a large box of the same sort of bones from the Ohio, by the way of Philadelphia. He informed me of this, and told me likewise that another large box of those bones was sent to the Earl of Shelburne, one of his Majesty's

Majesty's secretaries of state. I waited upon Dr. Franklin, with some other friends, and found the bones to be exactly such as I had seen; and was, therefore, confirmed in my former opinion.

Then I waited upon Lord Shelburne, and was permitted to examine the bones which he had received. Besides the tusks and grinders, which were all such as I had seen, and still served to confirm me in my opinion, there was the half of the lower jaw of the animal, with one large grinder still fixed in it. This jaw-bone was so different from that of an elephant, both in form and in size, and corresponded so exactly with the other bones, and with my supposition, that I was now fully convinced, that the supposed American elephant was an animal of another species, a *pseud-elephant*, or *animal incognitum*, which naturalists were unacquainted with. I imagined farther, that this *animal incognitum* would prove to be the supposed elephant of Siberia, and other parts of Europe; and that the real elephant would be found to have been in all ages a native of Asia and Africa only.

The Earl of Shelburne, from his love of natural knowledge, shewed a desire that the enquiry might be carried on; and did me the honour to offer his assistance in transmitting orders to America, for procuring farther information about this matter. In consequence of this generous offer, I proposed that his lordship should send the following questions and orders, to any person in America, whom his lordship might think the best qualified for conducting such business.

Queries and orders concerning the bones, called elephants bones, found in the marsh, called the Salt-Lick, near the River Ohio.

- I. Do those bones appear to have lain upon the surface of the earth from the first? Or,
- II. Do they seem to have been originally at some depth in the earth, and to have been afterwards exposed by the earth's falling away, or by its being washed away by floods, &c.?
- III. How far is that part of the marsh from the river? How high above the common surface of the water of the river? And does it appear probable, from the level and face of that marsh, that in former times the river may have run where the bones are?
- IV. How many elephants skeletons have been found, as far as may be collected from the number of tusks, or other marks? and at what distance from one another?
- V. To send over, if possible, a whole head, or the most entire parts of a head, especially of the upper jaw; and a foot, or the small bones of it, if they can be distinguished; and any bones which have those parts pretty entire which once made a joint.
- VI. To make correct drawings of any of the bones which are pretty entire, if, on account of their size, or tenderness, they cannot be sent over to England.
- VII. If the bones do not lie in blended heaps, but those of one single animal all together, and

at some little distance from others, it might be of service towards ascertaining the species of this animal, to expose or uncover one compleat sett of bones, without moving any one of them from its place; and to make a general drawing of the whole, as they appear in that situation; and to send as many of them as are tolerably perfect over to England, with that drawing.

Lord Shelburne was pleased to take the care of this proposal upon himself; and in proper time will probably receive such information as may be satisfactory.

I thought it would be adviseable, in the mean time, to collect all the information I could upon this subject; and to lay the result of such enquiries before this Society: that those who may have better opportunities might be invited to the subject, and no longer leave so capital an article of natural history uncertain.

I examined all the fossil teeth, as they are called, in the Museum of this Society, and the head and teeth of an hippopotamus. Then, with Dr. Knight first, and a second time with Dr. Solander, I examined all the fossil teeth, and all the jaw-bones, and teeth of elephants, and hippopotami, and other large animals, in the British Museum; and some likewise in private collections. In making this search, I met with grinders of the *incognitum* that were found in the Brazils and Lima, as well as in different parts of Europe.

At this time Lord Shelburne presented the largest of the American tusks, and the jaw-bone, and some grinders, to the British Museum; and his Lordship did me the honour to send me the smaller tusk, and two grinders.

I went to four of the principal workers and dealers in ivory, with whom I saw and examined many hundreds of elephants teeth. Though they all assured me, that the real elephants teeth have often a spiral twist, like a cow's horn; they could not shew me one tooth so twisted, in all their collections, at the time when I visited them. Three of them did me the favour to come to my house; and they gave it as their opinion, that my two American tusks were genuine elephants teeth. One of them was even positive that they were African teeth. Another worker in ivory cut through that tusk which Lord Shelburne gave me. It proved to be found on the inside. He assured me, that it was true elephantine ivory; and that workers in ivory could readily distinguish the genuine, by its grain and texture, from all other bony substances whatever. He polished it; we compared it with other pieces of genuine ivory; and indeed they appeared to be perfectly similar. His opinion was afterwards confirmed by another experienced worker in ivory. Yet their opinion, and what I saw with my own eyes, convinced me of this fact only, viz. that true or genuine ivory is the production of two different animals; and not of the elephant alone.

Having thus collected all the materials to which I could have access, I carefully read what the French Academicians Messrs. Buffon and Daubenton have written on this question, in the *Histoire Naturelle*,
 Vol. LVIII. G Tom.

Tom. XI. p. 86. &c. and p. 147. &c. Tom. XII. p. 63.; and *Memoires de l'Acad. Roy. des Sc. Ann. 1762.* p. 206. &c. But, instead of meeting with facts which could disprove my opinion, I found observations and arguments which confirm it. One very material fact, which Mr. Daubenton furnishes in support of my hypothesis, is the comparison of the American thigh-bone, with that of a real elephant; both of which he has represented in figures, which appear to be done with accuracy. To me it seems most evident, that they are bones of two distinct species. The vast disproportional thickness of the American bone, compared with that of the elephant, is surely more than we can attribute to the different proportions of bones, in the same species, which arise from age, sex, or climate. But Mr. Daubenton, to support his hypothesis, that the American *femur* is elephantine, is obliged to refer the great disproportion in thickness to the causes above-mentioned; and he affirms that in all other circumstances they are exactly alike. Now, to my eye, there is nothing more evident, than that the two *femora* differ widely in the shape and proportion of the head; in the length and direction of the neck; and in the figure and direction of the great trochanter: so that they have many characters, which prove their belonging to animals of different species.

In order to prove to the satisfaction of the society, that the *incognitum* of America is of a very different species from the elephant, I have added three drawings of the jaw-bone of that animal; which the curators of the British Museum were pleased to give me leave to take, and which Mr. Rymdyk executed with most scrupulous exactness: and that the comparison

II.

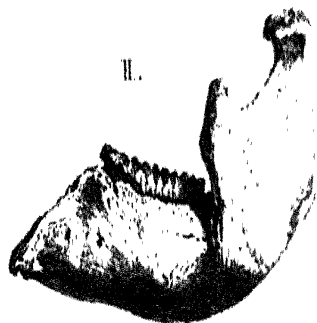
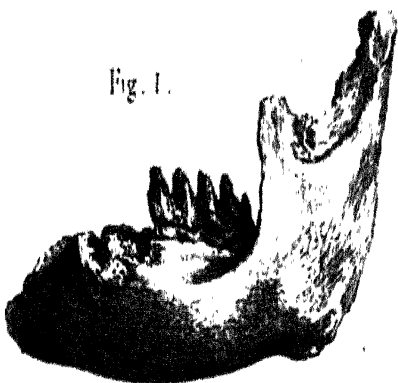
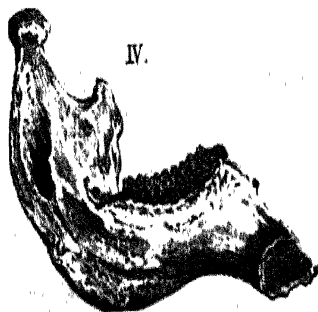


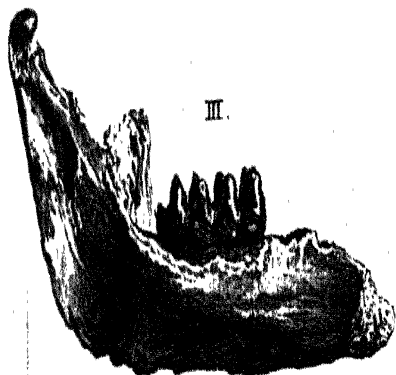
Fig. I.



IV.



III.



VI.



V.



parison might be made with ease, I have added three similar drawings, taken from the largest of the two full-grown Elephants jaws which were in my brother's collection; executed with the same care, by the same artist; and drawn to the same scale, *nine* inches in the real object making *one* in the figure.

TAB. IV. Fig. I. An outside view of the half of the lower jaw of the American *incognitum*, which the Earl of Shelburne deposited in the British Museum. From the top of the condyle to the anterior extremity, the bone measured, in a straight line, thirty five inches: the basis alone, in a straight line, two feet and four inches.

Fig. II. The same view of the same bone in a full-grown Elephant, drawn to the same scale.

Whoever will take the pains to compare these two figures, with a critical eye, will see that they differ so very much, not only in size, but in their general character, and in the particular parts and features, that he cannot entertain a doubt of their being the jaws of two very different animals.

Fig. III. A view of the inside of the same jaw-bone of the *incognitum*.

Fig. IV. A view of the inside of the same jaw-bone of the Elephant.

In comparing these two views, the difference if possible is still more manifest.

Fig. V. A view from above of the jaw of the *incognitum*.

Fig. VI. The same view of the Elephant's jaw-bone.

It may now be fairly presumed that the *Art* bones are proved to be *not* elephantine: and

and whoever is of that opinion, will naturally suspect that the Siberian bones are of the same kind. I imagine that it will be found, upon strict enquiry, to be so. But, as I have not the necessary materials for discussing this question at present, I shall only state a few facts, to shew that there is some ground for the opinion.

1. All accounts, and particularly those of Mess. Gmelin, Buffon, and Daubenton, say that the bones found in Siberia are larger than the bones of common Elephants. This would make us inclined to suspect that they were not Elephants bones, but that they were of the *Incognitum*.

2. The Siberian *femur*, as represented by Monsieur Daubenton, is *very much* like the American *femur* in size, shape, and proportions.

This circumstance appears to be almost a demonstration, as we have before proved, that the American *femur* is not that of an Elephant. And in this argument, we have even the weight of Monsieur Daubenton's opinion in our favour. For he (page 211.) taking it for granted that the Siberian *femur* was undoubtedly elephantine, *reasons* from the likeness in size, shape and proportions, that the American *femur* is so. Now, as we have shewn that the American *femur* is not elephantine, his proof taken from the size, shape, and proportions of the two bones, must serve to convince us that the Siberian thigh-bone is not of the Elephant, but of the *incognitum*.

3. Monsieur Daubenton found a difference between the temporal bone brought from Siberia, and that of an Elephant. This likewise is an argument in favour of our supposition.

4. The

4. The supposed Elephant's tusk, which was brought from Siberia by Mr. Bell, and presented to Sir Hans Sloane, and of which we have a description and figure in the Memoirs of the Academy of Sciences at Paris (An. 1727. page 309), is evidently twisted like the tusk of the *incognitum*, and not at all like any Elephant's tusk which I have ever seen. This proof will have considerable weight with those who will take the trouble to examine that tusk in the British Musæum.

In the last place, it may be observed, that as the *incognitum* of America has been proved to have been an animal different from the Elephant, and probably the same as the Mammouth of Siberia; and as grinder teeth like those of America have been dug up in various other parts of the world; it should seem to follow, that the *incognitum* in former times has been a very general inhabitant of the globe. And if this animal was indeed carnivorous, which I believe cannot be doubted, though we may as philosophers regret it, as men we cannot but thank Heaven that its whole generation is probably extinct.

Received January 11, 1768.

VI. *Observations made on the Islands of Saint John and Cape Briton, to ascertain the Longitude and Latitude of those Places, agreeable to the Orders and Instructions of the Right Honourable the Lords Commissioners for Trade and Plantations: By Captain Holland, Surveyor General in Canada, and his Assistants. Communicated by R. Brocklesby, F. R. S.*

Read March 3, 1768. **T**HE instruments made use of, in these observations, were;

I. A monthly astronomical clock, or time-piece (with a compounded pendulum, and a spring to keep it going when the clock is wound up), made by the late Mr. George Graham.

II. An astronomical quadrant, or equal altitude instrument of two feet radius, divided by Mr. Sisson, and improved, with an horizontal circle and stand, by Mess. Heath and Wing.

III. A two feet Gregorian reflecting telescope, made by Mr. Short.

IV. A ten feet refracting telescope, reversing the objects, made by Mr. Dollond.

In

In building my winter habitation on Saint John's Island, I constructed a strong stone chimney; to the back of which I secured the clock with the greatest precaution; and the room was kept temperate by an iron stove. In a few days, the clock was regulated to mean or equal time; and always examined and compared by equal altitudes of the sun and stars, at or near the time when any immersions or emersions were to be observed. As the going of this clock is not inferior to any made by that renowned artist Mr. Graham; it will not be necessary to insert here a multitude of equal altitudes of the sun, and other observations, to prove the exactness of this clock; but only mention, that I have made use of Monsieur De la Lande's Tables * to rectify the equal altitudes of the sun, for the alteration of the sun's declination during the time of observations.

1765.

1 Obser. January 20. Observed an emersion of the first satellite of Jupiter, at 7 hours, 42 minutes, 3 seconds, equal or mean time. Mr. Haldimand, who observed with Mr. Dollond's telescope, perceived the satellite two seconds later than I did with Mr. Short's; the latter having the second power magnifying 150 times. As this was the first observation, we began too soon to observe, and fatigued our eyes so much, that we were not sure to a few seconds.

* See Monsieur De la Lande's *Astronomie*, Article 637, page 279.

2 Obser. January 27. The first satelkite of Jupiter emerged at 9 hours, 36 minutes, 31 seconds, mean or equal time. By bringing Mr. Short's telescope out of a warm room, into the excessive cold air, the mirror became very dull, by which I lost my observation; but Mr. Haldimand was sure of his being good.

3 and 4 Obser. March 23. The third satelkite of Jupiter in immerging began to appear faint at 9 hours, 4 minutes, 48 seconds, and intirely immersed at 9 hours, 5 minutes, 47 seconds; and in emerging began to appear faintly at 12 hours, 27 minutes, 52 seconds, and fully recovered its lustre at 12 hours, 27 minutes, 58 seconds, equal or mean time. Observed by us both.

5 Obser. April 6. The first satelkite of Jupiter in immerging appeared faintly at 10 hours, 15 minutes, 54 seconds, equal time, and recovered its full lustre in 4 seconds afterwards. Observed by us both.

6 Obser. April 21. The first satelkite of Jupiter emerged at 8 hours, 34 minutes, 51 seconds, and recovered its full lustre in 3 seconds afterwards. Observed by us both.

7 Obser. April 29. The first satelkite of Jupiter emerged at 10 hours, 29 minutes, 30 seconds, equal time. Observed with Dollond's telescope only.

By the mean of all the meridian altitudes of the sun and stars; the latitude of this place is $46^{\circ} 2' 30''$ north.

In the beginning of May, I went on the survey of Saint John's Island; and lieutenant Haldimand
to

to survey and settle the latitude and longitude of the Magdalen Islands.

The longitude, lieutenant Haldimand determined by the mean result of several observations of the distance of the Moon, from the Sun and fixed stars.

This gentleman would have given a full account of his observations, with some natural remarks on the Sea Cows, which in prodigious numbers resort to that Island, had he not unfortunately lost his life, being drowned presently after his return to Louisburg.

The result of his observations is ;

The Magdalen Islands are situated in the Gulph of St. Lawrence, in the latitude of $47^{\circ} 41'$ North, and between 61° and $61^{\circ} 38'$ West longitude from London. The variation of the Compass is $17^{\circ} 30'$ West.

The Island of Entry, lies in $47^{\circ} 17'$ North latitude, and $61^{\circ} 20'$ West longitude from London.

The Bird Islands are in $47^{\circ} 55'$ North latitude, and bear from the East point of Magdalen North 35° East, distant 18 Miles.

Bryon Island is in $47^{\circ} 52'$ North latitude, and the East point bears North 13° West of the East point of Magdalen; distant 12 miles.

At my arrival at Louisburg, the 26th October 1765, I brought the instruments in order; fixed the clock to a brick wall, in a room kept warm by a stove, and regulated it to equal or mean time.

I also put up in a different room, near the fire place, another monthly clock, sent me from London, made by Mess. Mudge and Dutton, with a simple or common pendulum; this clock, with little trouble, was soon brought to keep time with the other clock, though,

when the fire was neglected, it was soon perceived in its motion.

The weather here proved very disadvantageous for astronomical observations, as well by the continual fogs, as severe frost.

1766.

1 Obser. March 10. Observed by Mr. Thomas Wright and me; the first satellite of Jupiter emerged at 11 hours, 21 minutes, 27 seconds, equal or mean time. This observation was made with the aforementioned instruments, and in the same manner. This day I had taken several equal altitudes to examine the clock, and found it 4 seconds too slow of equal time.

April 7. We discovered a Comet at 8 hours, 52 minutes; the tail of which was perpendicular to the horizon, with its head towards the Sun: the light of it was very pale, and it set behind the hills at 9 hours, 30 minutes. As near as I could remember, its position, when I came to look on the globe, was between the tail of Aries and Musca; we made preparations to make proper observations on the night ensuing; but a fog, which continued for several days, prevented us; and when it cleared up, the comet had so much approached the Sun, that we never after could see it.

2 Obser. April 14. The third satellite of Jupiter emerged at 11 hours, 51 minutes, 47 seconds; but some flying clouds made this observation a little dubious, which also occasioned losing the immersion of the same satellite. Observed by us both.

3. Obser.

3 Obser. April 25. The first satellite of Jupiter emerged at 11 hours, 46 minutes, 50 seconds. Observed by us both.

May 1. The several surveying parties dispersed round this Island, to finish the survey, and take the latitudes of all the most remarkable places; of which the result is;

At Louisburg and Island Battery, the latitude is $45^{\circ} 54'$ North.

Cape North, latitude $47^{\circ} 2'$ North.

St. Paul's Island, North cove, latitude $47^{\circ} 11'$ North.

The entrance of Dartmouth Harbor or *Baye des Espagnols*, latitude $46^{\circ} 13'$ North.

—Conway or St. Anne's Harbor, latitude $46^{\circ} 20'$ North.

The North head of Colvilles Bay, or Niganiche, latitude $46^{\circ} 44'$ North.

August 4. I returned to Louisburg, to observe the eclipse of the Sun on the day following. This eclipse appeared here perfectly annular; but I could only observe the undermentioned circumstances, as my large telescope and micrometer were out of order.

I observed with the telescope fixed to my quadrant or equal altitude instrument of two feet long reversing the objects; and Mr. Thomas Watts observed with one of Dollond's, four feet long.

During the time of observation, the sky was very clear, and I took six equal altitudes, to examine the clock.

	h	'	"	
The beginning of the eclipse	12	38	40	} Equal, or Mean Time.
The ring formed	2	8	48	
The ring opened	2	12	6	
End of the eclipse	3	33	50	
	H	2		From

1767.

From this time I was prevented, either by the business of the survey, or by the inconveniency of the weather, to make any further observations, until February 18.

1 Obser. The first satellite of Jupiter immersed at 11 hours, 47 minutes, 10 seconds, mean time.

2 Obser. February 25. The same satellite of Jupiter immersed at 13 hours, 41 minutes, 14 seconds, mean time.

3 Obser. February 27. The same satellite of Jupiter immersed at 8 hours, 10 minutes, 17 seconds, mean time.

4 Obser. March 6. The same satellite of Jupiter immersed at 10 hours, 4 minutes, 29 seconds, mean time.

5 Obser. March 15. The same satellite of Jupiter immersed at 8 hours, 41 minutes, 19 seconds, mean time.

6 Obser. April 7. The same satellite of Jupiter emerged at 8 hours, 53 minutes, 11 seconds, mean time.

7 Obser. April 14. The same satellite of Jupiter emerged at 10 hours, 47 minutes 44 seconds, mean time.

The observations of the immersions and emersions were this year made with Dollond's refracting telescope; the reflecting telescope of Mr. Short being sent with Mr. Wright (one of my deputy surveyors), who went last August to the Island of Anticosti, to survey, make observations for the ascertaining the longitude and latitude, and keep a meteorological journal.

I hope

I hope some of Mr. Wright's observations will be made on the same time with mine, that I may be able to settle immediately the longitude of the Island of Anticosti, and the entrance of the river of St. Lawrence. But to determine with exactness the longitudes hitherto taken, it will be necessary to deliver a copy of this paper to some members of the Royal Society, that they may be compared with corresponding observations, made in England.

By order of the Surveyor General.

George Derbage, Sec.

Received January 14, 1768.

VII. *A Note concerning the Cold of 1740, and of this Year.* By J. Bevis, M. D. F. R. S.

Read March 3, 1768. **I** Find in my journal of astronomical observations made at Stoke Newington, in a detached or insulated observatory, whose walls were of brick and two feet thick, that during most part of the night of the 5th day of January 17 $\frac{3}{4}$ the ink in my stand-dish would freeze in a few minutes, if brought within a foot of the wall; and that the surface of the water wherein the ball of the plumb-line hung, for rectifying the position of my mural quadrant, was continually freezing, so that I was obliged to thaw it frequently, by pouring in hot water; yet was there a good fire in the room all the night. At 5 in the morning, of the 6th, a Fahrenheit's thermometer, made by himself, exposed to the North, stood somewhat lower than 10, that is, more than 22 divisions below freezing. This was the coldest night of that year there.

This present year, in Brick-court, N^o 1, Middle Temple, the same thermometer, exposed out o'doors to the North, stood lowest on New year's day in the morning, to wit at 17, and once again at the same place; but then I am to observe that I am on that side my chambers invironed with buildings almost contiguous to the wall my instrument is hung against, wherein very probably fires were kept up, which hindered the quicksilver from sinking considerably lower.

J. Bevis.

Received February 4, 1768.

VIII. *Observations on the same Subject, by*
J. Short, F. R. S.

Read March 3, 1768. **I**N the year 1738, I bought of Mr. Scarlet a diagonal barometer. On the upright part of this barometer is affixed a spirit of wine thermometer, said to have been made according to the scale of the Royal Society's thermometer. On Tuesday, the 25th of December 1739, the great frost of that and the succeeding year began. On Saturday the 19th of December, at one o'clock in the afternoon, I observed that the spirits of wine were descended so low as not to be seen, that is to say, were below the top of the wood which covers the ball of the thermometer. I had another thermometer of quicksilver, made also after the scale of the Royal Society's thermometer: I found that the quicksilver of it was sunk within the ball. This last thermometer has been broke many years ago. The spirit of wine thermometer I have still in my possession, in the same condition it was then. This thermometer stood then, as it does now, within a room next to the river, at the greatest distance from two windows, one of which looks South, the other West. The wind blew hard from the East on the 29th

of December, There was then no ice on the river, which I could see.

I have since that time settled the freezing point of this thermometer. I find that the space, on this thermometer, contained between the freezing point and the top of the wood which covers the ball, is equal to 21 divisions of Fahrenheit's scale. It, therefore, follows that the cold, at one of the clock in the afternoon on the 29th of December 1739, was so great as to sink the mercury 21 divisions below the freezing point of Fahrenheit's scale, within this room, the windows being shut.

On the 31st of December 1767, at 8 o'clock in the morning, a Fahrenheit's thermometer without the window of the same room, where it had remained all night, stood at $19\frac{3}{4}$ divisions below the freezing point; but a similar Fahrenheit's thermometer within the room stood at $13\frac{3}{4}$ divisions below the freezing point; therefore the cold, that morning, was greater without the room than within it by 6 divisions of Fahrenheit's scale.

On the 7th of January 1768, at 8 o'clock in the morning, a Fahrenheit's thermometer without the window of the same room, where it remained all night, stood at $19\frac{1}{2}$ divisions below the freezing point; but a similar Fahrenheit's thermometer within the room stood at 12 divisions below the freezing point; therefore the cold, that morning, was greater without the room than within it by 8 divisions of Fahrenheit's scale.

From what has been said, I think, we may safely conclude, that the cold, on the 28th of December 1739 at one o'clock in the afternoon, was so great, without the

the window of the said room as to sink the mercury 27 divisions, at least, below the freezing point of Fahrenheit's thermometer.

N. B. No fires were made in the said room, or in the two contiguous rooms, in the year 1739, or in the years 1767 and 1768.

Surry Street, 3d
February, 1768.

J. Short.

Received January 18, 1768.

IX. *An Investigation of the Difference between the present Temperature of the Air in Italy and some other Countries, and what it was Seventeen Centuries ago: In a Letter to William Watſon M. D. F. R. S. by the Honourable Daines Barrington F. R. S.*

Dear Sir,

Read March 3, 1768. **I** Troubled you lately to procure from your Son Mr. Watſon, who is now in Italy, ſome answers to certain queries with regard to the temperature of the ſeaſons which commonly prevails there at preſent, and particularly whether the rivers of that country have been remembered to have had their ſurface frozen over.

I have often put the ſame queſtion to many of thoſe who have made the tour of Europe, and have always been answered in the negative: as moſt Engliſhmen however travel before they pay attention to facts of this ſort, I was deſirous of procuring information from thoſe whoſe obſervations might be more depended upon.

I have long entertained a notion that the ſeaſons are become infinitely more mild in the Northern latitudes than they were 16 or 17 centuries ago; and from this it hath happened that many paſſages in the claſſical writers deſcriptive of the Severity of the climates, have ſtruck me more than they would perhaps a common reader.

It

It will be immediately seen that the proofs of this assertion must depend upon accounts of the weather, and it's effects in places, the situation of which we know with some precision, and which may be compared with the common meteorological observations in the same latitudes and spots at present.

If this same question should be agitated two thousand years hence, it might receive an absolute demonstration; as a journal of the changes in a well-constructed thermometer would shew the temperature which prevailed in any particular place, during the present century.

No such accuracy can be expected from any passages in the classical writers; but in order to state the alteration which may have happened in so long a course of years, the most proper method seems to be to compare their accounts with those of more modern travellers, who have equally wanted the assistance of a thermometer for their observations.

I shall for several reasons chiefly rely upon many of Ovid's letters from Pontus (though he was not only a poet, but a writer of most glowing fancy, and imagination), in which he describes the effects of cold at Tomos* during his seven years residence there, and afterwards contrast this description with that of later travellers.

* It is so called by Ovid, who resided there so long and understood the language of that country. It was however likewise styled Tomis and Tomi, the latter of which seems to have been the more general appellation, as the adjective formed from it is Tomitanus. Besides this, Ferrarius supposes it to be the same with the modern Temisware, most evidently taken from the ancient name in the time of Ovid.

Ovid was born at Sulmo in Italy, about ninety Roman miles S. W. from the capital :

“ *Millia qui decies distat ab urbe novem.*”

He afterwards resided chiefly at Rome, and was there at the time he received the Emperor's orders for his immediate banishment: I shall therefore consider him as then leaving the 42d degree of Northern latitude, the climate in which he was born, and continued to live.

He was thence removed to Tomos, which Dr. Wells, in his maps of ancient geography, places only in the 44th degree of Northern latitude: the change was therefore only of two degrees, and yet Ovid immediately describes the winter of Hudson's bay.

But, before I particularise any of the passages which prove the intenseness of the cold, which he there experienced, it may be objected that no credit is to be given to a melancholy poet, of a warm imagination and too exquisite feelings.

This argument, I admit, would have great weight, if he only complained of the excessive and intolerable cold which prevailed. The maxim of law, however, holds equally in natural philosophy* “ *that he who means to impose or misrepresent never deals in particular facts,*” especially such as admit of an immediate contradiction, and in which he could not himself be deceived.

He saw with his own eyes the Euxine sea covered with ice :

“ *Vix equidem credar, sed cum sint præmia falsi*

“ *Nulla, ratam testis debet habere fidem :*

“ *Vidimus ingentem glacie consistere Pontum.*”

Lib. III. El. 10.

* “ *Fraus versatur in generalibus.*”

But if you will not credit what he saw, he afterwards mentions walking upon this ice :

“ Néc vidissè sat est, udum calcavimus æquor ;

“ Undaque, non *udo. sub pede*, summa fuit.”

The sea thus frozen, not only bore Ovid who hath described himself to be very light and agile, but oxen and carriages passed over it :

“ Perque novos pontes, subter labentibus undis,

“ Ducunt Sarmatici barbara plaustra boves.”

Lib. III. El. 10.

When the poor banished poet, during this rigorous weather, wanted some generous wine to warm himself, it was presented to him in a state of congelation :

“ Udaque consistunt formam servantia testæ

“ Vina, nec hausta meri, sed data frustra, bibunt.”

This effect of cold was not experienced in London, situated in the 52d degree of Northern latitude, during the great frost in 1740.

Add to these proofs, that what he here mentions is not the effect of one particular hard and severe winter ; he complains from year to year of nearly the same circumstances :

“ Ut sumus in Ponto *ter* frigore constitit Ister,

“ Facta est Euxini *dura ter* unda maris.”

Lib. I. El. 10.

The snow likewise in many places never dissolved during the summer :

“ Quæque nec hoste fero, nec nive terra cares.”

“ Frigore perpetuo Sarmatis ora riget.

“ Et solet in multis *bima* manere locis.”

I think therefore that what I have presumed to conjecture, may be rested upon the single testimony
of

of Ovid ; but, as there is with some so strong prejudice against facts from such a quarter, I shall endeavour to corroborate this authority, by descriptions of the same country, which we find in other writers of those centuries.

His contemporary Virgil speaks thus of the effects of cold in the same latitude :

“ Cæduntque securibus humida vina,
“ Stiriaque impexis induruit horrida barbis.”

Georg. Lib. III. l. 349. & seq.

He likewise asserts that the snow.

“ ——— Septem affurgit in ulnas.”

Virgil indeed is also unfortunately a poet ; but his Georgics are perpetually relied upon as authority, not only by Pliny, but the later writers on husbandry.

Such credit is given to our own great descriptive poet Thomson, that the compilers of the *Encyclopædie* have almost entirely translated his *Seasons*, under the article *Zone* : nor is there perhaps a circumstance mentioned throughout those poems, which the most scrupulous, and minute naturalist may not rely upon.

I shall just mention the authority of one more poet, as he is scarcely more than a metrical geographer :

Dionysius, in describing the same country, speaks thus of the snow's never melting :

Σχετλιοι αι περι κεινον ενοικια φωτες εχασι.

Αιαι σφιν ψυχρη τε χλων, δρυμας τε δυσανης.

Περιγηγησις, l. 668.

I shall close these corroborating proofs of the cold which was experienced at Tomos, by a passage from another geographer, who is sufficiently prosaic not to admit of any objection, to his testimony, on account of a too lively imagination :

Απασα

Ἀπασα δὴ χώρα δυσχεμερὲς ἐστὶ, τῶν δὲ παγῶν ἡ σφοδρότης μαλιστα ἐκ τῶν συμβαινόντων περὶ τὸ σῶμα τῆς Μαωτιδὸς δηλὸν ἐστὶ, ἀμαζεύεται γὰρ ὁ διαπλῆξ, ὥστε καὶ πηλὸν εἶναι, καὶ ὁδόν. Strabo, L. 7.

Thus is Ovid supported in that very material and striking fact of the ice being commonly strong enough to bear carriages.

It now remains to compare this account of the severity of the cold at Tomos, with that of more modern travellers, who have either been at the same place during the winter, or passed not very far distant.

Rubruquis, Marco Polo, Jean du Plan, Carpin, and Mandeville, were all of them on the borders of the Euxine Sea, and proceeded many degrees Northward; and yet we do not hear of any complaint with regard to the cold.

Busbequius travelled from Buda to Constantinople, in the midst of winter; nor does he mention any inconvenience, or interruption, from frost or snow. If it be said that his way did not lye through Tomos (or Temesware), to this it may be answered, that he crossed the same latitudes; to which it may be added, that there was no Euxine Sea to mitigate the severity of the cold.

I must likewise here make another observation, that it does not appear either from Ovid, who is so very minute in every particular relative to this country, or from any other traveller, that there are high mountains in the neighbourhood of Tomos.

Tournefort was on the Black Sea, in the beginning of April, and dwells much upon the very fine weather during the time he continued upon it. He observes, however, that, in the time of Constantine, the streight opposite to Byzantium was frozen over; and that

that in the year 401 the Euxine Sea was covered with ice, for twenty days together. These facts, therefore, struck him, as extraordinary.

Mottraye was in Crim Tartary, in the year 1711, during the months of November and December ; who is also entirely silent, with regard to any uncommon effects of cold.

These are all the travellers, whose works I have looked into, or could procure on this occasion. I do not take upon myself to say, that there may not be others, which have escaped me ; but I should not suppose the number to be great, as the Euxine Sea, and its neighbourhood, neither answers to the European traveller, in point of curiosity or commerce.

I have said in the outset, that I have some particular reasons, for fixing chiefly on Tomos, to make this comparison ; which arises from the country being precisely in the same state that it was in the time of Ovid ; this entirely excludes the common observation, *that the cultivation of a country will render the climate more temperate.*

We will now leave Tomos, and compare the accounts of the weather in Italy, with those of the present times : it being first premised, that the country was better cultivated, in the Augustan age, than it is now, which should consequently have made the temperature of the air more warm than it is now experienced to be.

The queries proposed to your son Mr. Watson relate to this comparison, and have occasioned my troubling you with this length of letter, since I have within these few days been fortunate enough to procure, through other hands, the information I could have wished on this head.

I shall

I shall begin with some passages from Virgil's Georgics, having already attempted to shew that no authority can be more relied upon.

This most excellent husbandman is constantly advising precautions against snow and ice in the management of cattle; and he may be generally supposed to give these directions for the neighbourhood of Naples *, or Mantua his native country, where he does not evidently from the context mean some other parts of Italy:

“ Et multâ duram stipulâ, flicumque manipulis

“ Sternere subter humum, glacies ne frigida lædat

“ Molle pecus.” Lib. III. l. 297.

This relates to sheep; but that hardy animal the goat wanted the same attention during the winter:

“ Ergo omni studio, glaciem, ventosque nivales

“ Avertes.” Lib. III. l. 317.

Speaking afterwards of Calabria, the most Southern part of Italy, he expresses himself, with regard to the rivers being frozen, as what was commonly to be expected:

“ Et cum tristis hyems etiamnum frigore fæta

“ Solveret, & glacie cursus frænaret aquarum †.”

* “ Illo Virgilium me tempore dulcis alebat

“ Parthenope, studiis florentem ignobilis oti.”

† It appears also by the sixth Satire of Juvenal, that the Tiber's being commonly frozen in winter supplied the ladies of Rome with a very extraordinary instance of implicit deference to the commands of the Egyptian priests:

“ Hybernū fracta glacie descendet in amnem,

“ Ter matutino Tiberi mergetur ————”

Pliny's favourite villa of Laurentinum was situated near the mouth of the same river; and, in the very minute description of its beauties and conveniences, he dwells much more upon the exposition of different parts of it to the warmth of the sun,

I am ashamed to be obliged to state so many authorities ; but, as the proof entirely arises from many such concurrent passages, I shall now support the testimony of Virgil by that of two naturalists, who were either Italians, or resided in Italy.

Pliny, in a chapter, *De natura cæli ad arbores*, and speaking of Italian trees, says, “ Alioqui arborum frugumque communia sunt, nives diutinas sedere.” Lib. XVII. cap. 2.

But perhaps the strongest proof of that very remarkable fact, the Italian rivers being constantly frozen over, is to be collected from a chapter in *Ælian*, which consists entirely of instructions how to catch eels, whilst the water is covered with ice: to this, without troubling you with a long citation, I shall barely refer. (See Lib. XIV. de Animal. cap. 29.)

Now, if we may believe the concurrent accounts of modern travellers, it would be almost as ridiculous to advise a method of catching fish in the rivers of Italy, which depended entirely upon their commonly being frozen over, as it would be to give such directions to an inhabitant of Jamaica.

I likewise cannot find that the precautions, which Virgil gives in his *Georgics*, against the damage which sheep and goats might receive from the snow and frost, are now necessary ; and both these animals are known to stand the severest winters of the High-

than its coolness, which is the circumstance most attended to, even in our northern climate.

He also mentions, that the situation was not warm enough either for olives or mirtle ; and that the *laurus* (which, whether it be the *bay* or *laurel*, bears our climate, except in seasons of extraordinary severity) would not then frequently stand the whole winter, neither at Laurentinum, nor near the town of Rome.

lands

lands of Scotland, conceived to be in Virgil's time almost the *ultima Thule*.

I do not pretend however to assert from this, that snow does not lye upon the Alps and Apennines, which arises from their very considerable height, nor that some waters on the tops of such mountains, or perhaps nearly under them, may not be frozen, especially when they are at a great distance from the sea.

We shall find however that the climate hath likewise, upon these very summits, become proportionably mild with that of the more level countries.

This receives a very clear and satisfactory proof, from the difficulties which those formerly encountered who passed the Alps.

Every line, almost, of both Livy's and Polybius's description of Annibal's passage, makes mention of frost and ice; we know that these mountains have been easily crossed since by armies, from the time of Francis the first, to the war of 1743. They are likewise passed sometimes even by consumptive travellers during the winter.

It is now time, perhaps, to release you from this very long dissertation; on a point, however, which seems to be of some curiosity and importance. I am,

Dear Sir,

Your most faithful,

humble servant,

Daines Barrington.

X. *An Account of Rings consisting of all the Prismatic Colours, made by Electrical Explosions on the Surface of Pieces of Metal, by Joseph Priestley, LL. D. F. R. S.*

Read March 10, 1768. **I**T was a discovery of Sir Isaac Newton, that the colours of bodies depend upon the thickness of the fine plates which compose their surfaces. He has shown that a change of the thickness occasions a change in the colour; differently coloured rays being thereby disposed to be transmitted through the plate, and consequently rays of different colours being disposed to be reflected at the same place, so as to present the appearance of different colours to the eye.

A variation in the density of the plate, he shows, will occasion a variation in the colour; but still a medium of any density would exhibit all the colours according to the thickness of the different parts of it. These observations he confirmed by experiments on plates of air, water, and glass. He also mentions the colours which arise on polished steel, by heating it; as likewise on bell-metal, and some other metalline substances, when melted and poured on the ground, where they may cool in the open air: and he ascribes these colours to the *scoriæ*, or vitrified parts of the metal, which, he says, most metals, when heated, or melted, do continually protrude, and send out to their surface, covering them in the form of a thin glassy skin. *Optics*, pag. 194.

This capital discovery, concerning the colours of bodies depending upon the thickness of the fine plates

plates which compose their surfaces, of whatever density those plates be (and which may be of such admirable use to explain the colours, and perhaps, in due time, the constituent parts and internal structure of natural bodies) I have been so happy as to hit upon a method of illustrating and confirming, by means of electrical explosions. These, being received upon the surfaces of all the metals, change the colour of them, to a considerable distance round the spot on which they are discharged, so that the whole space is divided into a number of concentric circular spaces, each exhibiting all the prismatic colours; and perhaps as vivid as they can be made in any method whatever.

It was not by any reasoning *a priori*, but by a mere accident, that I first discovered these colours. Having occasion to take a great number of explosions, in order to ascertain the lateral force of them; I observed that a plate of brass, on which they were received, was not only melted, and marked with a circle, by a fusion round the central spot, but likewise tinged, beyond this circular spot, with a green colour, which I could not easily wipe out with my finger. Struck with this new appearance, I replaced the apparatus, and continued the explosions; till, by degrees, I perceived a circle of red beyond the fainter colours; and, examining the whole with a microscope, I plainly distinguished all the prismatic colours, in the order of the rainbow. The diameter of the red, in this instance, happened to be one third of an inch, and the diameter of the purple about one fourth.

Pleased with this experiment, I afterwards pursued and diversified it in a great variety of ways, the result of which I shall comprise in the following observations:

1. When

1. When a pointed piece of metal is fixed opposite to a plain surface, the nearer it is placed to the surface, the sooner do the colours appear, the closer do the rings succeed one another, and the less space they occupy; as, on the other hand, the farther it is placed from the surface, the later do the colours appear; but the rings then occupy a proportionably greater space, and have more room to expand themselves. N^o 1. on the steel *, was made by the explosions passing from the point of a needle, fixed at the distance of $\frac{2}{3}$ of an inch from the steel; and N^o 2. was made at the same time, when the needle was placed at the distance of $\frac{1}{2}$ of an inch. It seems, however, that when the point is placed at such a distance, as that the electric matter has room to dilate, and form as large a circular spot as the battery will admit, the rings are as large as they are capable of being made; but that still the colours appear later, in proportion to the distance beyond that. When the point is fixed exceeding near, or is made to touch the surface, the colours appear at the very first explosion, but they spread irregularly, and make not distinct rings, as N^o 1. upon the tin.

2. The more acutely pointed is the wire, or needle, from which the electric matter issues, or at which it enters, the greater number of rings appear. A blunt point makes the rings larger, but fewer; and in that circumstance it is likewise much later before the colours make their appearance at a given distance. N^o 3.

* All the coloured rings mentioned in this paper were shewn to the Royal Society, but could not be well represented by a print,

upon the steel, was made by a blunt wire, and N^o 2. upon the tin by a brass knob fixed as before to 1.

3. In making these rings, the first appearance is a dusky red, about the edges of the circular spot; presently after which (generally after four or five strokes) there appears a *circular space*, visible only in a position oblique to the light, and looking like a shade on the metal. This space expands very little during the whole course of the explosions, and it seems to be, as it were, an attempt at the first and faintest red; for by degrees, as the other colours fill the bulk of that space, the edges of this shade deepen into a kind of brown; as may be seen particularly in N^o 4. upon the steel, where it is something more than half an inch in diameter, and in N^o 1. where it is near $\frac{3}{4}$ of an inch.

4. After a few more explosions, a second circular space is marked out by another shade, beyond the first, generally about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch in diameter, which I have never observed to change its appearance, after ever so many explosions. This second shade, by succeeding the first; which as I observed, becomes gradually of a brown, or a light red, seems to be an attempt at the fainter colours, which intervene between the reds.

5. All the stronger colours make their first appearance at the edges of the circular spot; and more explosions make them continually expand towards the extremity of the space first marked out, while others succeed in their place; till, after about thirty or forty explosions, three distinct rings generally appear, as in N^o 4. upon the steel. If the explosions be continued farther, the circle becomes less beautiful, and less distinct;

tinged; the red commonly prevailing, and suffusing all the other colours, as in N^o 1. upon the steel; though I attribute the confusion of the colours in that circle, in part, to the needle having been several times accidentally broken from the cement which supported it, and to its not having been replaced exactly as before.

6. The last formed colours are always the most vivid, as appears very distinctly in the reds of N^o 1 upon the steel. Also the last formed rings lie closer to one another than the first.

7. These rings may be brushed with a feather, and even wetted, or a finger may be drawn over them, without their receiving any injury; but they easily peel off, when scratched with one's nail, or any thing that is sharp, the innermost rings being the most difficult to erase.

8. The first circles are sometimes covered with a quantity of black dust; part of which however may be wiped off with a feather, so as to show the colours under it. An attempt to wipe off more, on the rough side of the steel, took off the colours along with it; but more than half yet remains, with the dust upon it, as it was first formed.

9. It makes no difference whether the electric matter issue from the pointed body upon the plate, or from the plate upon the pointed body; the plate opposed to the point being marked exactly alike in both cases. Also the points themselves, from which the fire issues, or at which it enters, are coloured to a considerable distance, often about half an inch, but not very distinctly. The colours likewise return here, in concentric rings, as upon the plate.

10. I think

10. I think that the more circles are made at the same time, the more delicate will the colours be; whereas the surface is, as it were, torn, or corroded by more violent explosions; which makes the colours appear rough and coarse. N° 4. is I think on this account, as well as some others, marked in a more delicate and beautiful manner than N° 1. or N° 5. But this roughness is only perceived on the steel. On silver, tin, and polished brass, the colours were always free from that roughness.

11. A polished surface is not necessary, the colours being very manifest on the rough side of the steel, where it is not covered with the black dust mentioned above.

12. These coloured rings appear almost equally well on all the metals on which I have made them; namely, gold, silver, copper, brass, iron, lead, and tin.

I have not tried any of the semi-metals; but I have no doubt of their answering as well as the proper metals.

13. When the pointed wire was made to incline to the plane on which the colours were exhibited, the circular spot was quite round, the center of it being in the perpendicular let fall from the point; but the colours were projected opposite to the point, in an oblong figure.

Upon shewing these coloured rings to Mr. Canton, I was agreeably surprised to find, that he had, likewise, produced all the prismatic colours from all the metals, but by a different electrical process. His method had been to extend fine wires over the surface of pieces of glass; and when the wire was exploded, he observed that the glass remained tinged

with all the colours from all the metals. They are not indeed disposed in so regular and beautiful a manner as in the rings I produced; but they equally demonstrate, that none of the metals discovers the least preference to any one colour more than another. A variety of other very extraordinary appearances occurred in the course of Mr. Canton's experiments in melting wires.

In what manner these colours are formed, it may not be easy to conjecture. In Mr. Canton's method of producing them, the metal seems to be dispersed in all directions from the place of explosion, in the form of spheres, of a very great variety of sizes, tinged with all the variety of colours, some of them too small to be distinctly visible by any magnifier. In my method, it should rather seem that they are produced in a manner similar to the production of colours on steel &c. by heat *i. e.* the surface is affected, without the parts of it being removed from their places, certain plates only, or *laminæ*, being formed, of a thickness proper to exhibit the respective colours at certain distances; and that the thickness of these plates is continually changing by the repetition of the explosions.

N.B. The *battery* made use of in the abovementioned experiments was of *twenty one square feet* of coated glass.

Received March 9, 1768.

XI. *A Letter from John Ellis, Esquire, F. R. S. to the President, on the Success of his Experiments for preserving Acorns for a whole Year without planting them, so as to be in a State fit for Vegetation, with a View to bring over some of the most valuable Seeds from the East Indies, to plant for the Benefit of our American Colonies.*

Read March 10,
1768.

HAVING discovered that the disappointment, which I met with about a year ago, in attempting to preserve through the season some ever-green oak acorns and some chefnuts in wax, was owing to their being unfit for vegetation at the time of my inclosing them; I resolved in my next attempt to try only such as I was persuaded were sound and fresh.

Fortunately, my curious and learned friend the Right Honorable Sir Thomas Sewell Master of the Rolls, hearing of my distress, offered to procure me some excellent acorns of the english oak, part of a parcel he had been sowing at his seat at Ottershaw near Chertsey in Surrey; these he was so obliging to send me the 20th of February 1767; part of them I sowed immediately under the windows of my chambers, in the kitchen garden of Grays Inn: and on the 22d of the same month I inclosed about 36 of them in bees-

wax. Most of those that I had sown in the garden came up in June following 1767, and by the middle of September were 6 inches high.

This gave me some hopes that I should not labour in vain as I had done before; for part of the same parcel of ever-green oak acorns which failed, I had given to Sir Thomas Sewell the year before to sow, and he assures me that not one of them came up with him. I likewise discovered, though too late, that the Spanish chesnuts, which failed, had been kiln-dried; this is a common practice in Spain, to prevent their sprouting by the damp heat in the hold of the ship.

I should not, my Lord, be so particular in explaining the cause of my disappointment, but to shew the care that is necessary to be taken by persons abroad in the choice of the seeds, as well as the state they ought to be in, if they expect they should answer the great end we propose.

Before I mention the method in which I treated these acorns, I must observe to your Lordship, that though I have formerly been so successful as to preserve both acorns and chesnuts for the space of a year in bees wax, several of which have afterwards vegetated, and some of them are now grown into trees; yet I always found that many of them were rotten when they were taken out of the wax; which made me suspect that it was owing to the too great heat of the melted wax, that so many of them were destroyed. This put me on thinking of the following method to guard the seeds to be preserved from too great heat, which I have now the pleasure to shew your Lordship and the rest of the Royal Society the good effects of.

After

After I had chosen out the fairest acorns, laying aside such as had specks proceeding from the wounds of insects, I wiped them very clean till they were quite bright, for fear of any condensed perspiration on the surface, which if inclosed, would turn to mouldiness. I then poured some melted bees-wax into a china plate about half an inch deep, and soon as the wax was cool, but still very pliable, I cut out with a penknife as much as would inclose one acorn; this I wrapped round it, rolling it between my hands till the edges of the wax were perfectly united: in the same manner I covered about 36 of them with all the caution in my power, so that after they had been set to harden I could not perceive the least crack in them. When they were quite cold and hard, I prepared an oval chip box, of 7 inches long, $4\frac{1}{2}$ broad, and $3\frac{1}{2}$ deep; into this I poured melted bees-wax to the depth of an inch and half; and when I could bear my finger in it, I laid the covered acorns at the bottom in rows as close as I could together; afterwards other rows over them, till the box was full; and when the first wax began to cool, I poured some wax that was barely fluid over the uppermost acorns till they were quite covered. In order to cool them as soon as possible, I set the box near a window, where the sash was raised a little to let in a stream of cold air; when they were almost cold, I perceived the wax had shrunk a little here and there, and left some chinks; these I immediately filled up with very soft wax, pressing it very close and smooth. After it was quite cold and hard, I put on the cover of the box, and placed it on a shelf in a closet till the beginning of August last, when I sent it to the care of Mr. Dacosta, clerk to the Royal Society, to their house in
Crane

Crane Court, to be produced and examined before the Royal Society at some of their first meetings after the long vacation. My health would not permit me to attend myself; but I am informed, my Lord, that when they were cut open and examined before your Lordship and the rest of the Royal Society present, their appearance promised success; and that they were ordered to be delivered by Dr. Morton, secretary to the Royal Society, to the care of Mr. William Aiton, Botanic Gardiner to her Royal Highness the Princess Dowager of Wales at Kew at my request, that the Royal Society might be informed whether they would vegetate.

I have just now, my Lord, had the pleasure of receiving a letter from Mr. Aiton, advising me, that he had sent to Mr. Robertson, housekeeper to the Royal Society, two pots with the young oaks rising from the acorns preserved in wax, which Dr. Morton sent him from the Royal Society in December last; and am well persuaded he has carefully attended to an experiment, the success of which, if properly followed, may in a few years put us in possession of the most rare and valuable seeds in a vegetating state from the remotest parts of the world, which in time may answer the great end of the improvement and advancement of our trade with our American Colonies. I am,

My Lord,

with the greatest respect,

Your Lordship's

most humble and obedient servant,

Gray's Inn, March 9,

1768.

John Ellis.

SIR,

Kew, March 8, 1768.

S I R,

I Received December 5, 1767, of Doctor Morton of the British Museum a parcel of acorns preserved in wax (the quantity of acorns which I received was 34); and according to your desire and direction they were sowed, as soon as I received them, into a sandy light loom. I placed the pots with the acorns under a frame, where they remained till January the 28. I then took the pots with the acorns out of the frame, and placed them near a window, in one of our large airy stoves, where they have remained ever since; according to your desire, they shall be sent to-morrow to the Royal Society's house. I think the gentlemen of that Honourable Society will be pleased to see the method of preserving seeds in wax prove so successful; as the acorn is one of the worst of seeds to keep any time, out of the ground, from perishing; and the good success there is from those few which I received from Doctor Morton. I am, therefore, of the opinion, that, if seeds are sound and dry, and carefully put up in the wax, it is the best method that has ever been found out to preserve seeds from distant countries.

I am, Sir,

Your most obedient and humble servant,

To John Ellis, Esq;
Gray's Inn.

William Aiton.

N. B. There are sixteen in one pot, and nine in the other, that are already come up, and most of them from four to six inches high.

XII. *A Letter from Dr. Donald Monro, F. R. S. to Mathew Maty, M. D. Sec. R. S. inclosing one from Mr. Farley, of Antigua, on the good Effects of the Quassi Root in some Fevers.*

S I R,

Read March 17,
1768.

AS we have had no further accounts of the Quassi Root, since Dr. Linnæus published the sixth volume of his *Amœnitates Academicæ* * in the year 1764, I have, according to your desire, sent you the copy of a letter on the good effects of this root, which I hope will be acceptable to the Society, as it may excite Physicians to make trials of this medicine which seems to promise to be of so much use. The original letter was given me by the gentleman to whom it is addressed, while I attended him last year when he was here in England for the benefit of his health. I am,

S I R,

Your most obedient humble servant,

Jermyn-Street, March 8,
1768.

D. Monro.

* Dr. Carol. Linnæus gives a particular description and figure of the Quassi tree, which grows in the neighbourhood of Surinam, in South America, and of the Root having been administered at Surinam, with great success, in malignant, remitting and intermitting fevers: and he tells us that its virtues were first discovered by a slave of the name of Quassi, from whom the tree got its name.

Copy of a Letter from Mr. James Farley, Practitioner in Physick in the Island of Antigua, to his Partner, Mr. Arch. Gloster, in London; dated Antigua, July 26, 1767.

S I R,

MR. T—r has been extremely ill since his arrival with a fever, which lasted for many hours; and, upon its going off, he could not retain the bark in any shape whatever. Many things were tried to check the vomiting, and enable him to keep down some bark, but to no purpose. At last I tryed the Quassi Root, an account of which I read in one of the magazines for this year; it sat extremely well on his stomach; he had no vomiting after the first dose, and recovered very speedily.

I have lately tried it in three or four cases, where there has been a tendency to putrefaction and the bark would not stay on the stomach; a dram of this root, has effectually answered every purpose that the bark would. It has this advantage over the bark, that it does not heat the patient.

I have given it in fevers, joined with the Radix Serpentariæ Virginianæ, with success. I had a pound or two from Esquebo, and have sent you a little of it.

Dr. Warner has sent Dr. Jackson a piece of it; he saw the good effects of this medicine, in a patient, Captain B—n, who sails for London to-day. He attended him with me. I could not get the bark to fit

on his stomach, for he had a perpetual vomiting, and could not keep down any nourishment whatever. I prepared a decoction of a dram and a half of the Quassi Root, and a dram of the Rad. Serpent. Virgin. When it was ready, I sent for Dr. Warner, that he might see the patient before I administered it; he complained of some pain on touching the pit of his stomach, had a very sluggish low pulse, a great pain over his eyes and in his eye balls, and vomitings. He took the decoction, which surprizingly put a stop to his vomiting; he had no return after the first dose, and kept down every thing. We indeed gave him some Camphor and Sal Succini, on account of the sluggishness of the pulse, but I have tried it alone in a decoction, with infinite advantage.

Signed, J. Farley.

Received January 27, 1768.

Read March 10, 1769.

XI. Meteorological Observations for 1767, made at Carlisle, Bridgewater, and Ludgvan; and communicated by the Bishop of Carlisle, F. R. S.

OBSERVATIONS made by Dr. CARLYLE at CARLISLE in CUMBERLAND,

N. B. Under column 1. stand the months. Column 2. contains an account of the barometer lowest morning and evening stations of the mercury, in the month, included within the farbers are put before any station, it denotes it to have been alike on these several days. Column 3. contains the thermometer; noted in like manner as the barometer. Column 4. contains an account of the hygrometer; the day of the month on which the greatest quantity of rain fell that month, and what that quantity was; the second the total depth of rain in the month. Column 5. contains the state of the wind

the highest and
or more num-
account of the
the first line notes
inches and deci-
&c.

Month.	Barometer.	Thermometer.	Hygrometer.	State of the
January	<p>Highest. 4. Morn. 30.1 4. Even. 30.125 Lowest. 11. Morn. 28.925 11. Even. 29.05</p>	<p>Highest. 31. Morn. 45 31. Even. 47 Lowest. 10. Morn. 25 10. Even. 25</p>	<p>0.935 22. Total 1.647</p>	<p>The month opened with very stormy winds from the North; succeeded by frost, and some snow; but on the 10th began to fall the deepest and longest continued snow remembered here; it was measured to full 14 inches deep, on the level, in Carlisle, in the evening of the 11th; and the ground remained covered thus with it till the 26th and 27th; while the first snow fell, the wind was North; when the great one fell it was South-West, and very high. The remainder of the month it was chiefly southerly. Though the snow went off remarkably gently, yet it occasioned two very great floods, on the 26th and 7th; till which time, the thermometer had kept mostly considerably under 40. The 10th it was the lowest for the whole day I ever remember it.</p>

M 2

February

State of the Winds, Weather, &c.

Baromet.		ter.	Hygrometer.	State of the Winds, Weather, &c.	
February	High.	High.			Weather pretty mild and open, but cloudy, with frequent rains. A flood on the 17th. Wind till the 8th, from the South; then northerly nigh a week; afterwards mostly in the southern points. On the 26th a storm from South-West.
	25. Morn. 29.95	1, 2, 4, 5. Morn. 47	24.	0.41	
	24. Even. 29.95	2, 3, 4, 11. Even. 48	Total	2.426	
	Lowest.	Lowest.			
March	14. Morn.	1, 19, 21, 28. Morn. 43			Weather cloudy, with small rain till the 6th; when it became fair and frosty till the 15th; the middle of the month was very wet, and snow fell on the hills; the latter end was mild and fair. Winds mostly in the West quarter till the last week, when it turned easterly. The barometer was in general high.
	26. Even.	28. Even. 43	18.	0.366	
	High.	High.	Total	1.586	
	30. Morn. 30.125	31. Morn. 50			
April	9. Even. 30.125	31. Even. 51			Barometer in general very high through this month; the weather fair and dry. On the 16th and 17th very hard frost; on the 18th ice one-tenth of an inch thick on the roads; on the 19th the hills covered with snow. Wind in the beginning from South and East; afterwards often from the North and East.
	Lowest.	Lowest.	Total	0.211	
	22. Morn. 29.6	15. Morn. 35			
	22. Even. 29.7	14. Even. 38			
May	High.	High.			Barometer lower in general than last month. Weather favourable till the 9th; rainy and moist till the 13th; dry and duly till the 24th; after this rainy, till the end. Wind in the middle of the month North and East. Its beginning and end South and West. Cuckow heard on the 12th.
	1. Morn. 30.475	10, 14. Morn. 52	1.	0.135	
	1. Even. 30.4	30. Even. 55	Total	0.211	
	Lowest.	Lowest.			
June	30. Morn. 29.225	17, 18. Morn. 43			Weather to the 14th bright and fair. Winds North or East. After that frequent showers, to the end of the month, from the North-West or South-West quarters.
	29, 30. Even. 29.3	6, 17, 18. Even. 46	30.	0.908	
	High.	High.	Total	3.141	
	9. Morn. 30.4	3. Morn. 58			
July	9. Even. 30.4	18. Even. 60			
	Lowest.	Lowest.			
	1. Morn. 29.575	6. Morn. 47			
	1. Even. 29.575	5, 7. Even. 49			
August	High.	High.			
	9. Morn. 30.4	11. Morn. 61	24.	0.174	
	9. Even. 30.4	11. Even. 62	Total	0.559	
	Lowest.	Lowest.			
September	1. Morn. 29.575	5. Morn. 47			
	1. Even. 29.575	5. Even. 47			
	High.	High.			
	9. Morn. 30.4	11. Morn. 61			

Month.	Barometer.	Thermometer.	Hygrometer.	State of the Winds, Weather, &c.
July	<p>Highest. 30.05 17. Morn. 30.05 16. Even. 30.05 Lowest. 29.95 4. Morn. 29.25 3. Even. 29.15</p>	<p>Highest. 62 20. Morn. 62 19, 20, 30. Even. 62 Lowest. 56 2, 6, 16. Morn. 56 1. Even. 55</p>	<p>6. 0.414 Total 5.941</p>	<p>Very few days quite fair this month, yet the rain fell so much in showers, or in the night, the hay harvest was not much interrupted by it. Wind mostly from the South or West points. Thunder on the 12th and 20th, with violent rains to the East of us. On the 23d great lightnings.</p>
August	<p>Highest. 30.2 27, 28. Morn. 30.2 27. Even. 30.225 Lowest. 29.95 15. Morn. 29.375 14. Even. 29.35</p>	<p>Highest. 68 5. Morn. 68 4, 29. Even. 68 Lowest. 55 19. Morn. 55 19. Even. 55</p>	<p>14. 0.296 Total 2.03</p>	<p>Weather very variable, as were the winds, with frequent and sudden changes of showers and sunshine; towards the end of the month more settled weather. Barometer in general low. Thunder, with severe showers, on the 4th. N. B. The thermometer at Red Castle, in the open air, hung against the North wall of the tower, on the 4th rose to 80 at noon; at 1 p. m. was at 78; at 2, 77; at 3, 76.</p>
September	<p>Highest. 30.45 21. Morn. 30.45 21. Even. 30.45 Lowest. 29.55 7. Morn. 29.55 4, 6. Even. 29.575</p>	<p>Highest. 65 21. Morn. 65 21, 24. Even. 64 Lowest. 54 30. Morn. 54 30. Even. 54</p>	<p>14. 0.552 Total 3.065</p>	<p>The wind mostly southerly. On the 6th thunder with severe showers. From the 16th to the 26th bright clear weather, without a shower. Some blustering winds from the North-West in the end of the month.</p>
October	<p>Highest. 30.35 19, 20. Morn. 30.35 19, 20. Even. 30.35 Lowest. 29.9 4. Morn. 29.9 4. Even. 29.15</p>	<p>Highest. 58 26. Morn. 58 24, 26. Even. 57 Lowest. 43 9, 11. Morn. 43 8. Even. 43</p>	<p>7. 0.572 Total 2.957</p>	<p>Wind mostly in the West. The 2d and 4th very stormy from the West and North-West. On the 8th a flood, and the hills covered with snow. The 9th and 10th hard frost, with thick ice upon the waters; which was succeeded by rain. From the 18th were high winds from the South-West for nigh a week. The 29th, 30th, and 31st, were also very stormy from the same quarter.</p>

Month.	Barometer.	Thermometer.	Hygrometer.	State of the Winds, Weather, &c.
November.	Highest. 28. Morn. 30.3 23. Even. 30.225 Lowest. 14. Morn. 28.65 14. Even. 28.95	Highest. 10. Morn. 55 9. Even. 54 Lowest. 16. Morn. 38 15. Even. 41	14- 0 593 Total 4.084	Winds mostly from the South or West, with almost constant rains till the 16th; inasmuch that we had a remarkable high flood on the 7th; and three other very considerable ones on the 10th, 12th, and 14th. On the 14th the hills all round were covered with snow. We had very stormy winds on the 10th, 11th, and 13th. On the 14th hail, thunder, and lightning: the lightning had been very frequent in the nights, particularly from the 8th, and continued to the 22d, though thunder was rarely heard. On the 15th a smart frost set in for three days. On the 19th was another, but less flood; from thence cloudy and showery to the end of the month. On the 26th coughs were almost universal among the horses, both in town and country; though very few died.
December.	Highest. 3. Morn. 30.575 2. Even. 30.55 Lowest. 7, 8. Morn. 29.45 7. Even. 29.45	Highest. 16. Morn. 50 15. Even. 49 Lowest. 31. Morn. 26 30. Even. 27	14- 0.203 Total 0.624 Tot. for the Y. deep; which, coming with hard frost, remained on the ground 26.268 to the end of the month.	Winds in the West to the 16th. Weather cloudy and showery, with snow on the hills. From the 16th to the end 0.624 bright, with frost. Winds from the East. Snow began to fall on the 24th; on the 27th it fell 2 inches and a quarter deep; which, coming with hard frost, remained on the ground to the end of the month.

G. Carlye.

OBSERVATIONS

OBSERVATIONS at BRIDGWAT SC ERSE

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermometer.	Ombrometer.
January 14.	Highest 30 Lowest 28	The first part of the month frosty. Wind N. E. and N. W. The latter part stormy, with high winds, and variable from N. W. to S. W. The 11th the snow was 6 inches deep, undriven.	Highest 53 Lowest 15	In. Dec. 1 ,255
February 24.	Highest 30 Lowest 29	Stormy throughout. The wind in general either S. W. or S. E.; and in the latter part of the month high.	Highest 51 Lowest 36	3 ,372 $\frac{1}{2}$
March 6.	Highest 30 Lowest 29	The first part cloudy and sunshine alternately. The latter part intermixed with storms. The wind generally S. W. in the first part; and variable to N. W. and S. E. in the latter.	Highest 51 Lowest 34	1 ,802 $\frac{1}{2}$
April 20.	Highest 30 Lowest 29	The first and last parts settled, with sunshine and cloudy intermixed. From the 20th to the 26th some rain. The wind generally N. E.	Highest 53 Lowest 37	1 ,217 $\frac{1}{2}$
May 1.	Highest 30 Lowest 29	The first part settled fair. The middle stormy, and the latter part changeable. The wind for much the greater part N. W.	Highest 59 Lowest 41	1 ,147 $\frac{1}{2}$
June 8.	Highest 30 Lowest 29	The first week stormy. The remainder settled fair. The wind N. W. or N. E.	Highest 67 Lowest 43	,920
July 27.	Highest 29 Lowest 29	Unsettled, and rainy in general. The wind almost constantly S. W.	Highest 67 Lowest 53	4 ,107 $\frac{1}{2}$ August

Month.	Barometer.		State of the Weather and Wind.	Fahrenheit's Thermometer.		Ombrometer.
	In. Dec.			Deg.		In. D.c.
August	27. Highest 14. Lowest	30.15 29.44	The first week settled fair. The remainder unsettled and rainy. The wind generally N. W. and gentle.	4. Highest 17. Lowest	66 52	2,870
September	21. Highest 4. Lowest	30.32 29.50	The first part changeable and stormy. The latter part settled fair. The wind variable. The first and latter part rather strong; in the middle gentle.	4. Highest 29. Lowest	64 52	2,245
October	13. Highest 3. Lowest	30.34 29.18	From the 1st to the 9th windy and stormy. Settled to the 17th; and from thence to the end unsettled. The first part the wind generally N. E. and N. W. The latter part S. W.	26. Highest 13. Lowest	62 39	3,572 $\frac{1}{2}$
November	28. Highest 14. Lowest	30.37 28.08	The first part unsettled, with high winds. The latter part cloudy and foggy, with gentle wind. In the beginning and latter part the wind N. W.; and in the middle generally S. W.	9. Highest 17. Lowest	59 34	1,932 $\frac{1}{2}$
December	3. Highest 19. Lowest	30.57 29.23	To the 22d generally cloudy; and from thence to the end frosty. The wind through the whole month gentle, and generally either N. E. or N. W. At 10 A. M. On the 3d of this month the mercury in the barometer stood at 30.59.	9. Highest 34. Lowest	72 57	2,27 $\frac{1}{2}$
Total of Rain				24		8.50

OBSERVATIONS

OBSERVATIONS at LUDGIAN, in MOUNT BAY CORNWALL.

1767. Month.	Barometer. Nontus's Division.	General State of the Weather and Wind.	Fahrenheit's Therm. high, low, and Med. of three Obs. each Day; the Therm. constantly exposed in the Air, never to the Sun.	Omb.
January	In. Dec. 1. Highest 30.17 2. Lowest 28.50	Colder than usual. On the 5th snow began to lie upon hard frost. On the 11th snow near 8 inches deep in plains. On the 13th a quick thaw; and on the 14th 8 a. m. snow and frost all gone. Wind mostly N. and E. Stormy.	27. Highest 48 } 12. Lowest 31 } Med. 41 $\frac{5}{8}$	In. 3 7 $\frac{2}{5}$
February	24. Highest 29.91 30. Lowest 28.89	Misty, stormy, rainy. On the 12th a storm from midnight to 4 a. m. On the 20th a violent storm, with much rain. Wind mostly W.	7. Highest 49 } 23. Lowest 42 } Med. 46 $\frac{3}{4}$	6 370
March	5. Highest 30.8 27. Lowest 29.57	Cloudy. Calms. In the middle, for two days, fleets, calms, and frost; rest variable, calm, wind, and rain, variable.	31. Highest 50 } 16. Lowest 39 } Med. 45 $\frac{1}{2}$	3 600
April	30. Highest 30.26 23. Lowest 29.35	Misty. Fair. Calms. Winds mostly N. E. and W. nearly equal.	26. Highest 51 } 19. Lowest 43 } Med. 47 $\frac{3}{5}$	2 000
May	1. Highest 30.27 31. Lowest 29.15	Variable.	26. Highest 57 } 6. Lowest 47 } Med. 53 $\frac{5}{8}$	1 900
June	8. Highest 30.20 2. Lowest 29.37	Variable; but mostly mists and calms. Winds generally had some point of the N.	25. Highest 67 } 4. Lowest 46 } Med. 57 $\frac{3}{4}$	0 500
July	1. Highest 29.90 8. Lowest 29.34	Frequent rain and showers. Wind mostly W.	22. Highest 62 $\frac{1}{2}$ } 5. Lowest 50 } Med. 57 $\frac{1}{2}$	5 775

Month.	Barometer. Nontius's Division.	State of the Weather and Wind.		Fahrenheit's Thermometer. Highest, Lowest, and Medium, of three Observ. each Day.		Omb.
	In. Dec.	Calm.	Sunshine.	Showery.	Winds variable,	nearly
August	26. Highest 30 .10 29. Lowest 29 .45	equal.				
September	20. Highest 30 .24 4. Lowest 29 .17	Variable Winds; the West prevailing, with sunshine mornings,				
October	13. Highest 30 .13 4. Lowest 29 .20	Rainy. Calms. Windy; mostly from the West				
November	28. Highest 30 .26 16. Lowest 28 .93	Misty showers and rain. Stormy. Thunder and lightning, with showers; the extreme at 11 p. m.				
December	3. Highest 30 .42 19. Lowest 28 .80	Calm mists. In the middle of the month stormy with rain. On the 19th a storm. On the 20th ditto; reit hard frost with some snow.				
Total of Rain this Year at Ludgvan						37 $\frac{100}{1000}$

William Borlase.



XIV. *Account of the different Species of the Birds, called PINGUINS, by Thomas Pen-
nant, Esquire, F. R. S.*

P I N G U I N S.

Read March 17, 1768. **T**HE characters of this genus are, very small wings and those covered with meer shafts. Four toes on each foot, three of which are webbed, the fourth loose and standing forward.

I. The PATAGONIAN PINGUIN.

TAB. V.

Size. The length of the stuffed skin, we measured, was four feet three inches; and the bulk of the body seemed superior to that of a swan.

Bill. Four inches and a half long; slender, strait, bending only on the end of the upper mandible, black, covered on each side the base with soft short brown feathers; the sides of the lower mandible compressed, the lower part or base orange coloured, the end dusky. No nostrils.

Tongue. Half the length of the bill, and singularly armed with strong sharp spikes pointed backwards.

N 2

Plumage.

Plumage. The most remarkable of all the feathered tribe, each feather lying over the other, with the compactness of the scales of fish; their texture is equally extraordinary; the shafts broad and very thin; the vanes unwebbed; the head, throat, and hind-part of the neck, are of a deep brown colour; from each side of the head to the middle of the fore-part of the neck are two lines of bright yellow, broad above, narrow beneath, and uniting half way down; from thence the same colour widens towards the breast, fading away till it is lost in pure white, of which colour is the whole under side of the body, a dusky line dividing it from the colour of the upper part; the whole back is of a very deep cinereous colour, almost dusky; but the end of each feather is marked with a cœrulean spot, those about the junction of the wings larger and paler than the others.

Wings. Are extremely short in respect to the bulk of the bird, hang down, and have rather the appearance of fins, whose office they perform*; their length is only fourteen inches; on the outside they are dusky, and covered with scale-like feathers, or at best with such whose shafts

*De Veert's Voyage, p. 333. Winter's Voyage in Hacluyt's Coll. III. 752.

are so broad and flat as scarce to be distinguished from scales; those on the ridge of the wings consisting entirely of shaft; the larger or quill feathers have some very short webs.

Tail. Consists of thirty brown feathers, or rather thin shafts, resembling split whale-bone, flat on their upper side, concave on the under, and the webs short, unconnected, bristly.

Legs and Feet. } From the knees to the end of the claws
 } six inches, covered with strong pentangular black scales; the fore-toe scarce an inch long, and the others so remarkably short, as to evince the necessity of that strength of the tail, which seems intended as a support to the bird in its erect attitude; in the same manner as that of the wood-pecker is when it clings to the sides of trees; between the toes is a strong semilunar membrane, continued even up part of the claws; the middle claw is near an inch long, and the inner edge very sharp and thin; the interior toe is small, and placed very high.

SKIN. Extremely tough and thick, which, with the closeness of the feathers, guards it effectually in the element it is so conversant in.

History. This bird was brought by Capt. MacBride, from the Falkland Isles, off the Straits of Magellan; we believe this species

species to have been undescribed; for the birds that bear the same name are mentioned by every writer, who treat of them as far inferior in size to this; some compare their bulk to that of a duck; but none make it larger than a goose; the colours also of this species are too striking not to have been taken notice of, had it been before discovered.

Captain Mac-Bride was so obliging as to inform us that this was a very scarce species; though he saw in the same place multitudes of the lesser kind, with which it agreed in its manner of life. Since the natural history of each species is the same, we shall give a general view of the œconomy, &c. from such writers who have treated of them.

It is agreed that they are inhabitants of southern latitudes only, being, as far as is yet known, found only on the coasts of South America, from Port Desiré to the Straits of Magellan; and, if we remember right, Frezier says, they are found on the western shore, as high as Conception. In Africa they seem to be unknown, except on a small isle near the Cape of Good Hope, which takes its name from them.

They are found in prodigious numbers on land during the breeding season; for they seldom come ashore but at that time;

time; they form burrows under ground, like rabbits; and the isles they frequent are perfectly undermined by them, so that it is difficult to walk without falling into their holes, or sinking through the surface up the shoulders. Such rencontres are disagreeable, as these birds bite extremely hard; and commonly three or four are found to nestle together in the same hole.

Their eggs are said to be rather less than that of a goose; and that they begin to lay the latter end of September, or beginning of October.

Their attitude on land is quite erect; and on that account they have been compared by some to pygmies, by others to children with white bibs*.

On land they are excessively awkward, by reason of the situation of their legs, which are placed quite behind: they are very tame, and may be drove like a flock of sheep; when pressed, they seek for shelter either in their burrows, or the sea, which seems to be their more natural element.

In the water they are remarkably active, and swim with vast strength, assisting by their wings, which serve instead of fins.

Their food in general is fish, not but that they will eat grass like geese; for Sir

* Narborough's voyage, p. 59.

Richard Hawkins observed, in an isle they frequent off Patagonia, a small valley covered with grass, which the birds never burrowed in, as if they meant to reserve it for pasturage.

They are very fat, but taste very fishy, not unlike our puffins: as they are very full of blood, it is necessary to cut off their heads as soon as they are killed, in order that it may run out; it is also requisite they should be flayed, for without those precautions their flesh is scarce eatable. When salted it becomes a good food, as navigators have often experienced, in particular Richard Hopkins*, who preserved that way sixteen hogheads, which lasted above two months, and served as beef.

These birds and seals seem to have been bestowed in quantity on those desolate shores, as resources in extremity to distressed voyagers.

Name. The proper name of these birds is Pinguin (*propter pinguedinem*†), on account of their fatness. It has been corrupted to Penguin; so that some, imagining it to have been a Welsh word signifying a white head, entertained some hopes of tracing the British colony, said to have migrated into America, under the auspices of Madoc Gwineth, son of Owen

* Sir Richard Hawkins, Obs. 72.

† Clus. Exot. 102.

Gwineth, A. D. 1170 *. But as the two species of birds that frequent that coast have black heads, we must resign every hope founded on that hypothesis of retrieving the Cambrian race in the new world.

We give this species the epithet Patagonian, not only because it is found on that coast, but because it as much exceeds in bulk the common kinds, as the natives are said to do the common race of men.

I must not quit this subject without making my acknowledgements to Mr. Banks for communicating this curious bird to me, which he now permits to be laid before the Society for their examination.

II. The lesser P I N G U I N.

Anser Magellanicus. Clus. Exot. 101.

Black-footed Penguin. Edw. 94.

Diomedea demersa. Linn. Syst. 214.

Size. Of a goose.

Bill. Strait to the point, where it grows hooked ; the end of the lower mandible abrupt, as if cut off ; both are black, but marked across near the ends with a yellow bar.

Plumage. The crown, hind-part of the head, the cheeks, and chin, are dusky ; from the

* Powel, Hist. Wales, p. 229.

bill over the eyes, and then down to the neck, passes a white line; the back, and outside of the wings, are of the same colour with the head; the breast, belly, and sides, are white, marked with a brown line passing over the upper part of the breast, under the wings, and terminating at the legs.

They vary in colour; some wanting the white line over the eye, and the brown one over the breast.

Wings. Like those of the preceding.

Legs. Black, which agrees with the Magellanic goose of Clusius, and may be the lesser species observed by our late voyagers to the coast of Patagonia.

III. The red-footed P I N G U I N.

The Penguin. Edw. 49.

Le Gourfou Calaractes. Brisson. Av. VI. 102.

Phaeton demersus. Lynn. Syst. 219.

Size. Inferior to the last.

Bill. Thick, arched, and red.

Plumage. Like in texture to that of the former; the head, hind part of the neck, and the back, of dusky purplish hue; breast and belly white.

Wings. Brown, but the tips of the larger feathers white.

Tail. None, in lieu of it a few black bristles.

Legs. Red.

History.

History. This seems to be the African species ; for all that have described the South American kinds attribute to them black legs.

This is found on Penguin isle, near the Cape of Good Hope, of which Sir Thomas Roe, in his Voyage to India, gives this brief relation :

“ On the isle of Penguin is a sort of fowl
 “ of that name, that goes upright ; his
 “ wings without feathers, hanging down
 “ like sleeves faced with white ; they
 “ do not fly, but walk in parcels,
 “ keeping regularly their own quarters *.”

Left the bird known, by the name of Penguin, in the northern parts of Europe and America, should be confounded with these, it may be observed, that it is of another genus ; and it is by the later ornithologists very justly ranked with the Auks.

* In Churchill's Coll. of Voyagé, vol. I, p. 767.

XV. *The Application of Dr. Saunderson's Theorem for solving unlimited Equations, to a curious Question in CHRONOLOGY: By Mr. James Horsfall, F. R. S.*

Read March 24, 1768. **B**Y old tables it appears that Easter day happened on the 22d of March (which is the *soonest* it ever can happen), in the years of Christ 165, 697, 1229, and lastly in 1761.

Quest. 1. What is the next year of our Lord, when it will happen so again before 1900? For,

Note. From thence to 2199, the paschal full moon, or the golden number 14, which distinguished the years above, will be fixed on the 22d of March; consequently EASTER day cannot happen before the 23d of March in that period.

Answer. In the act for altering the stile, it appears by the table for finding Easter till 1899, that this can *never* happen in *that* period, but when the golden number, or lunar cycle, is 14, and the Sunday letter D.

Also, by making a *solat* cycle for that century, the first year of it will fall on 1812, the Sunday letters E D, wherefore all the years in that cycle, which have D for the Sunday letter, are 1, 7, 18, 24: and now the question is reduced to this.

Quest.

Quest. What year of our Lord in the 19th century will have the solar cycle either 1, 7, 18, or 24, when the lunar cycle is 14? Or, which is the same thing;

Quest. What number is there between 1800 and 1900, which divided by 28 leaves 1, 7, 18, or 24; and being divided by 19 leaves 14?

S O L U T I O N.

Here then in the general theorem $\frac{ar}{l} \times d - e + d$, is $a=28$, $b=19$, $D=1, 7, 18, \text{ or } 24$; $E=14$, $l=1$. To find r , the quotients are $* 1, 2, 9$; $* 19)28(1$ drop the first and last, because their number is odd: then the series required will be 0, 1 2; therefore $r=2$.

N. B. If to any year of Christ be added 9, and the sum divided by 28; the remainder, or 28, if 0 remains, will be the *cycle of the sun* for that year; and if 1 be added to any year of Christ, and the sum divided by 19; the remainder, or 19, if 0 remains, is the *cycle of the moon*. Hence, if any year of Christ be severally divided by 28 and 19, and the remainders be d and e respectively; then $d + 9$, or $d + 9 - 28 = D$; and $e + 1$, or $e + 1 - 19 = E$. In the present case, taking $D=1$, $d + 9 = 1$ cannot be; because d would be *negative*: but d must *not only be affirmative*, but also *GREATER* than e ; wherefore make $d + 9 - 28 = D = 1$; therefore

therefore $d = 1 + 28 - 9 = 25$; and $e + 1 = E = 14$, therefore $e = 13$.

D and E are the *cyclar* numbers, and d and e are the *anno domini* numbers suited to the theorem.

Here then $\overline{ar \times d - e} + d = \overline{28 \times 2 \times 7} + 20 = 412$; and $412 + 532 + 532 + 532 = 2008$. The first answer, therefore, A. D. 412, is too little, and when encreased by three *dionysian* periods, or *multiples* of 28 and 19 is too big, going beyond the century required. So, *when this solar cycle is 1, it will not do.*

Let $D = 7$, the rest as before. Then $d + 9 - 28 = 7$; therefore $d = 26$. Here then $\overline{ra \times d - e} + d = \overline{28 \times 2 \times 13} + 26 = 754$; and $754 + 532 + 532 = 1818$. So A. D. 1818 WILL ANSWER THE QUESTION.

Let $D = 18$, the rest as before. Then $d + 9 - 28 = 11$; therefore $d = 37$. Here $\overline{ra \times d - e} + d = \overline{28 \times 2 \times 24} + 37 = 1381$; and $1381 + 532 = 1913$. This goes beyond the century required; so *will not do.*

Let $D = 24$, the rest as before. Then $d + 9 = 24$; therefore $d = 15$. Here $\overline{ra \times d - e} + d = \overline{28 \times 2 \times 2} + 15 = 127$; $127 + 532 \times 4 = 2255$; which goes *beyond* the century required.

So there is *but one year* in the 19th century, *viz.* 1818, that will have the conditions required. The cycle of the sun will then be 7; the cycle of the moon 14; and the Sunday letter D; and *Easter Day the 22d of March*.

N. B. For

N. B. For *every century* a *new solar cycle* must be made; because, by the Act of Parliament*
 - for correcting the calendar, every 100th year for three centuries is *common*, and *not bissextile*; so that the same dominical letter stands against the same year, in the cycle only for 100 years in *three* successive centuries.

N. B. By a continual addition of 28 to 1700 or 1756 †, we have the *first* year of each solar cycle; and when the *first* year of that cycle *next after the beginning* of any century is had, and its dominical letter found, by the rules and tables in the act, *the cycle for that century may be formed*, with the dominical letters answering to each year of it; where-
 by may be seen on *what* years of the cycle *the same Sunday letter recurs*. Thus;

Quest. 2. If it was required to find in what years between 2200 and 2300 Easter Day would again happen on 22d of March; I find by the hints above, that the *first* year of the solar cycle falls on 2204; and, being *leap-year*, I find by the rules and tables in the act, that the *dominical-letters* are A G: from thence I construct the solar cycle of 28 years, as in table I.

And from the table prefixed to the late Earl of Macclesfield's Letter to Martin Folkes, Esq; P. R. S. read May 10, 1750, and published in Phil. Transf. Vol. XLVI. p. 47. shewing the place of the golden

* 24 George II.

† Vide Table I.

numbers in the calendar, and the paschal full moon, and the Sunday letter, answering thereto for that century (which stand as in table II), I construct table III, for finding Easter Day during *that* century; and observe it *never* happens on the 22d of March, but when the *golden number C*, and the *dominical letter D*.

And the dominical letter D happens only in the 4th, 9th, 15th, and 26th years of the solar cycle in that century, as appears by table I.

Now the question is reduced to this, *viz.*

What number is there between 2200 and 2300; which, being divided by 28, leaves either 4, 9, 15, or 26; and being also divided by 19, leaves 6?

S O L U T I O N.

In the general theorem above, *viz.* $\frac{ar}{l} \times d - e + d$ are given $a = 28$, $l = 1$, $r = 2$, as before; and to find the values of d and e ,

We have $d + 9 - 28 = D = 4$; therefore $d = 23$ } in the theorem :
And because $e + 1 = E = 6$; therefore $e = 5$ }

viz. $28 \times 2 \times 18 + 23 = 1031$; and $1031 + 532 + 532 = 2095$: so that this cyclar number will *not* do, the year falling either below or beyond the century required.

2. Let $D = 9$; the rest as before. Then since $d + 9 - 28 = 9$; therefore $d = 28$, and $e = 5$ as before; and $28 \times 2 \times 23 + 28 = 1316$; and $1316 + 532 + 532 = 2380$. This cyclar number will *not* do, for the same reason as the last.

3. Let

3. Let $D = 15$; the rest as before. Then since $d + 9 = D = 15$; therefore $d = 6$, and $e = 5$, as before; and $28 \times 2 \times 1 + 6 = 62$; and $62 + 532 \times 4 = 2190$. This cyclar number will *not* do, for the same reason as before.

4. Let $D = 26$; the rest as before. Then $d + 9 = 26$; therefore $d = 17$, and $e = 5$, as before; and $28 \times 2 \times 12 + 17 = 682$; and $689 + 532 \times 3 = 2285$; and this is the *only* year that will answer the question; because it has 6 for its golden number, and D for its dominical letter. Whence we may conclude, that after A. D. 1761, there will not be so long a Trinity-vacation again till 1818; and after that year, the like will not happen till 2285.

TABLE I.
Solar Cycle,
&c. for the
23d Century.

1756	112	4	28
1868	112		
1980	112		
2092	112		
2204	AG	1	
5	F	2	
6	E	3	
7	D	4	
8	CB	5	
9	A	6	
10	G	7	
11	F	8	
12	ED	9	
13	C	10	
14	B	11	
15	A	12	
16	GF	13	
17	E	14	
18	D	15	
19	CB	16	
20	BA	17	
21	G	18	
22	F	19	
23	E	20	
24	DC	21	
25	B	22	
26	A	23	
27	G	24	
28	F	25	
29	D	26	
30	C	27	
31	B	28	
32	AG	1	

TABLE III.
For finding Easter from 2207 to 2299, by the Golden
Number and Dominical Letters.

G. No	A	B	C	D	E	F	G
I Apr. 16	17	18		19	20	21	22
II Apr. 9	10	11		5	6	7	8
III Mar. 26	27	28		29	30	31	25
IV Apr. 16	17	18		19	13	14	15
V Apr. 2	3	4		5	6	7	8
VI Mar. 26	27	28		22	23	24	25
VII Apr. 16	10	11		12	13	14	15
VIII Apr. 2	3	4		5	Mar. 30	31	Apr. 1
IX Apr. 23	24	18		19	20	21	22
X Apr. 9	10	11		12	13	7	8
XI Apr. 2	Mar. 27	28		29	30	31	Apr. 1
XII Apr. 16	17	18		19	20	21	15
XIII Apr. 9	10	4		5	6	7	8
XIV Mar. 26	27	28		29	30	24	25
XV Apr. 16	17	18		12	13	14	15
XVI Apr. 2	3	4		5	6	7	1
XVII Apr. 23	24	25		19	20	21	22
XVIII Apr. 9	10	11		12	13	14	15
XIX Apr. 2	3	4	Mar. 29		30	31	Apr. 1

TABLE II.
Lunar Cycle,
&c. for the
23d Century.

Gold. Numbers.	D. of the Month for Paschal Full Moons.	Domin. Lett.
	6 Mar. 21	C
	22	D
	23	E
14	24	F
3	25	G
11	26	A
	27	B
19	28	C
8	29	D
	30	E
16	31	F
5	Apr. 1	G
	2	A
13	3	B
2	4	C
	5	D
10	6	E
	7	F
18	8	G
7	9	A
	10	B
15	11	C
4	12	D
	13	E
12	14	F
1	15	G
	16	A
9	17	B
17	18	C
	19	D
	20	E
	21	F
	22	G
	23	A
	24	B
	25	C

XVI. *A Determination of the Solar Parallax attempted, by a peculiar Method, from the Observations of the last Transit of Venus: By Andrew Planman, Professor of Natural Philosophy, in the University of Aböa, and Member of the Academy of Sciences at Stockholm; together with a Letter from him to Mr. James Short, F. R. S.*

Vir Celeberrime;

Read March 24,
1768.

DUM ante binos annos, in Transactionibus Philos. anni 1763, quæ in Bibliotheca Regiæ Academiæ Scientiarum Stockholmenfis servantur, animadverti te, vir celeberrime, exquisitissimam collocasse operam in investiganda parallaxi solis; mihi proposui, mearum lucubrationum, in eadem ipsa re, tibi quantocyus facere copiam: ast negotiorum multitudine distentus, hoc propositum differre cogebar ad hoc usque tempus.

Adest quidem aliquod discrimen parallaxium, quas obtinuimus: sed adscribendum est id, partim diversis observationum combinationibus, partim quoque diverse assumtis locorum longitudinibus, quas minime e re fore duxi, observationum conciliandarum ergo, immutare; quippe quas, etiam uno eodemque loco captas, nimium quantum discrepare

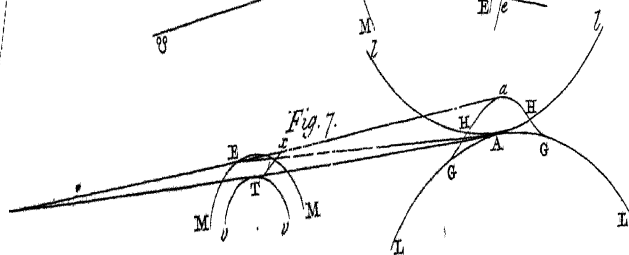
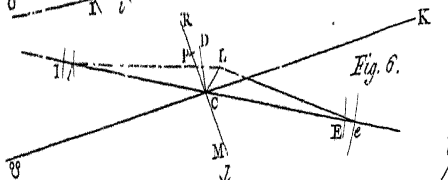
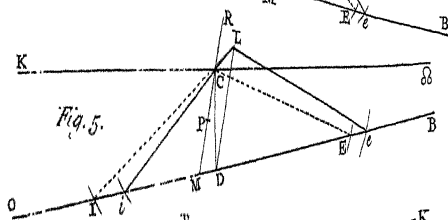
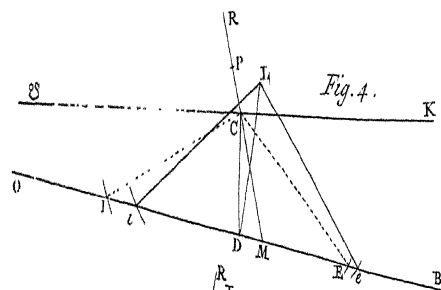
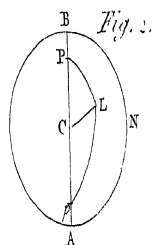
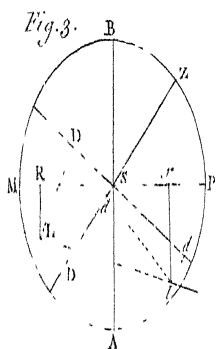
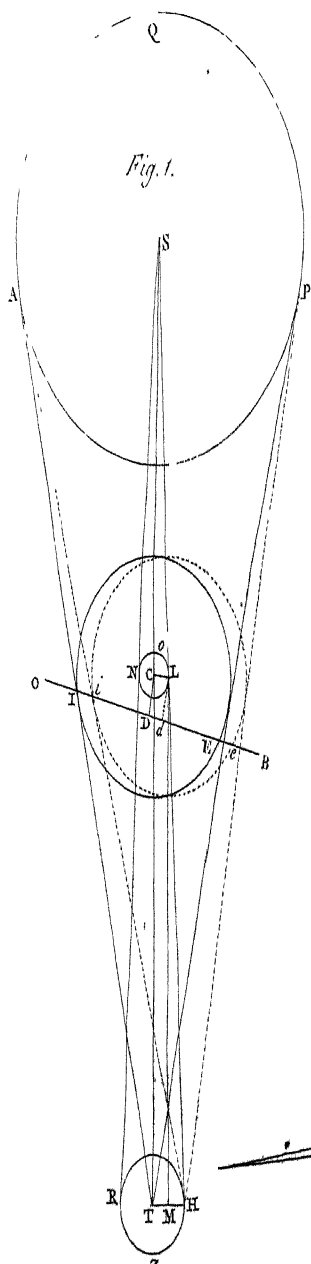
deprehendimus. Nec errorem in observando cuiquam facile imputaverim: ut enim taceam diversitatem tuborum, oculorum, aliarumque circumstantiarum; atmosphæra ista, qua Venerem cinctam esse jam novimus, non potuit non diversa exhibere ingressus atque egressus phænomena, aliis hoc, aliis illud pro vero ingressus aut egressus phænomeno habentibus; id quod § VI. ulterius expositum invenies. Si itaque meletemata mea digna esse judicaveris, quæ actis vestris inferantur, erit id mihi gratissimum, imprimis, ut methodus generalis, quam hic fisto, et supputationes, quas passim sparsimque exhibui, uno loco coacerventur. Molimina, quæ vestros astronomos jam detineant, ad excipiendam Venerem in proximo ipsius cum Sole congressu, æque gratum mihi foret rescire. Ex decreto Reg. Academiæ Scientiarum Stockholmenfis, ego Cajaneburgum iterum petam; ast celeberrimus Upsaliensium Astronomus Mallet usque ad Pello in Lapponiam contendet.

De cetero vivas diu ad vota felix; ego vero permanebo celeberrimi nominis tui

Affiduus cultor,

Dabam Aboæ, die 18
Decembris, anni 1767.

Andreas Planman.



Parallaxis Solis ex Observationibus novissimi Transitus Veneris per discum Solarem, peculiari quadam methodo investigata, ab Andrea Planman, Physices Professore in Academia Aboënsi, Academiæ Scientiarum Stockholmenfis Socio.

S E C T. I.

UT methodum * expeditam fisterem, qua parallaxis solis, ex observationibus Veneris Solem trajicientis, investigari posset; rem sequenti modo concipiendam duxi. Scilicet fingo mihi e centro telluris T (TAB. VI. fig. I.) rectas esse ductas ad singula puncta disci solaris P Q A, quem, absque notabili errore, sub toto transitu fixum supponere licet, atque has rectas in plano ad rectam T S, centra solis tellurisque jungentem, normali ac transeunte per O B apparentem planetæ semitam, e centro telluris visam, efficere disci solaris projectionem I K E, cujus centrum est in C, semitam O B in punctis I et E secantem. Si nunc ex C recta C D ita demittatur ad semitam O B, ut sit ad eclipticam normalis; videbitur, spectatori ex T, planeta in conjunctione cum sole quoad eclipticam, centro ipsius in D existente; ast dimidia disci sui parte immersus aut emersus spectabitur planeta, dum centro suo ad I aut E pervenerit. Si autem ex

* Hanc methodum initio anni 1763, in Dissertatione mea, de *Venere in Sole visa*, anno 1761, primum evulgavi et quidem ita, ut præcipue respicerem hujus novissimi transitus casum. Nunc autem illam hic generalem reddere atque ad singulos casus hujusmodi transituum extendere, e re omnino fore existimavi.

alio quocunque puncto disci terrestris, ex gr. ex H, ad finitorem lucis constituto, spectetur sol, indeque pariter agantur rectæ ad singula disci solaris puncta, mutabitur situs projectionis solis pro magnitudine et positione rectæ TH aut eidem parallelæ et proportionalis CL; adeo ut centrum solis S non jam in C, sed in L sit conspicuum; quapropter mutabuntur quoque dicta momenta, prout literæ minusculæ monstrant, existentibus *Ii*, *Dd*, *Ee*, effectibus parallaxis, qui utique determinandi sunt, quoties observationes horum momentorum in H factæ ad centrum telluris revocentur. Ut autem hoc facili negotio conficiatur, concipio ulterius, per rectas, ex centro solis S ductas ad singula disci telluris H R Z puncta, factam esse in eodem plano, quo sol est projectus, hujus disci projectionem NoL, qui itidem est circulus, cujus radius CL = *parallaxi horizontali planetæ a sole*; manente TH = *parallaxi horizontali planetæ*: nam ducta LM parallela ipsi ST, erit, ob angulos TSH et MLH æquales et valde exiguos, MH = *parallaxi solis horizontali*; adeoque TM = TH — MH = CL. Si punctum H fuerit, non in finitore lucis, sed alibi in disco terræ soli obverso, erit CL aut exacte aut quam proxime æqualis *parallaxi altitudinis planetæ a sole*, prout discrimen altitudinum planetæ et centri solis fuerit aut nullum aut admodum exiguum. Effectus itaque parallaxis pendet a diverso centri disci solaris situ in circulo LON, vel, quod eodem recidit, a diverso loco observatoris in hemisphærio HRZ; quippe hujus loci atque centri disci solis, ex hoc eodem loco conspiciendi, projectio coincidit in unum idemque punctum circuli LON. Quapropter quæstio, de æstimandis parallaxis

[III]

effectibus, eo est reducta, ut, pro tempore quovis dato, determinetur, respectu ipsius CD , cujuscunque dati et in circulum LoN projecti loci telluris situs, qui locus in posterum L vocetur. In hunc finem, oportet, inveniatur CL , una cum angulo LCD , intercepto a projectura circuli verticalis et circuli latitudinis. Cum autem iste angulus pendeat ab angulo parallactico, qui a meridiano et circulo verticali comprehenditur; erit hic angulus primum inveniendus.

S E C T. II.

Exhibeat itaque circulus ANB (fig. 2.) hemisphaerium telluris illuminatum et dicto modo projectum, in planum per semitam planetæ transiens (§ 1.), cujus radius $CB =$ parallaxi horizontali planetæ a sole; exhibeat quoque AB projecturam meridiani cœlestis, in qua sit polus aut boreus P , aut australis p , prout conjunctio planetæ ad aut ☿ aut ♀ facta fuerit. Sit quoque C commune centrum projectionis telluris et solis e centro telluris conspiciendi; nec non L projectio loci cujuscunque dati. Atque fiat latitudo loci $L = L$; complementum declinationis solis CP vel $Cp = D$; angulus horarius CPL vel $CpL = A$; sinus totus $= 1$; altitudo solis pro loco et tempore dato $= C$; nec non $\cos.$

$$A. \cot. L = \tan. G; \text{ eritque } \sin. C = \frac{\sin. L \cos. (+D \mp G)}{\cos. G}$$

(1), in qua signa inferiora tenenda sunt, quoties fuerit $D < G$, alias superiora valent, excepto casu, dum $A > 90^\circ$, quo signum — ipsius G abit in +, adeo ut summa ipsarum D et G sit accipienda. Statuatur

tuatur ulterius ang. parallaëticus PCL vel $pCL = Q$,
prodibitque fin. $Q = \frac{\text{fin. } A. \text{ cof. } L}{\text{cof. } C}$ (II).

S E C T. III.

Quod attinet ipsam CL , erit ista saltem quam proxime æqualis parallaxi altitudinis planetæ a sole (§ 1), nisi differentia altitudinum centri solis et planetæ fuerit vix negligenda; quo casu, ratio habenda est hujus differentiæ, quippe quæ parallaxin nominatam ab ista CL aliquantum discrepantem præbet. Interim tamen, etiam in hocce casu, parallaxin altitudinis planetæ a sole, absque notabili errore, in ipsa CL æstimare licet. In hunc finem differentia altitudinum centri solis siderisque jam est quærenda. Exhibeant igitur AB meridianum cælestem; PM (fig. 3.) parallelum æquatoris; ZN , zn circulos verticales loco et tempori respondentes; Ll loca quæcunque data planetæ ante et post conjunctionem ascensionalem; SR , sr , differentias ascensionis rectæ centri solis S atque planetæ, quas dico a ; nec non LR , lr , differentias declinationis, quæ vocentur d . Dicatur ulterius angulus, quem recta, jungens centra solis et planetæ, facit cum parallelo æquatoris, nempe ang. LSR vel lSr , F ; eritque ang. $F = \frac{d}{a}$; nec non distantia centrorum solis siderisque SL vel $Sl = \frac{d}{\text{fin. } F}$. Si nunc ex L , l , agantur normales LD , ld in circulos verticales ZN , zn ; erit SD vel sd differentia altitudinum quæsitæ, quæ dicatur E ; huic autem determinandæ inserviet formula

formula $E = \frac{d. \sin. (T \pm Q)}{\sin. F}$ (III.), in qua dabitur

T per d et a , quæ ex observationibus facile eliciuntur, et Q per æquationem (II). Circa signa autem sequentes regulæ probe sunt tenendæ: nempe *signum + valet, si observatio facta sit ante meridiem et ante conjunctionem, vel post meridiem et post conjunctionem in ascensione recta. Ast signum — est adhibendum in observationibus antemeridianis post conjunctionem, et post meridianis ante conjunctionem eandem.*

Hæc signa sunt invertenda, adeo ut signum — in prioris, et + in posterioris regulæ casu valeat, quoties semita planetæ fuerit borealior centro solis; uti fit in proximo Veneris transitu. Dabitur itaque jam, pro loco et tempore dato, per æquationem (I) et (III) altitudo planetæ, quippe quæ = $C \mp E$: ubi signum — in prioris, ex + in posterioris regulæ casu adhibendum est; exceptis illinc locis disci telluris australioribus centro solis, et hinc eodem centro borealioribus locis, dum dant $F \mp \text{compl. } Q \geq 90^\circ$; atque hæc exceptio probe erit observanda, quoties semita planetæ, ad alterutrum nodum, fuerit australior centro solis. Si vero planeta latitudine boreali trajiciat solem, exceptio locorum respectu est invertenda, adeo ut signum + in casu prioris regulæ valeat, quoties locus disci telluris, solis centro borealior exhibuerit $F \mp \text{compl. } Q \geq 90^\circ$; si vero idem fiat, in casu posterioris regulæ, respectu puncti terrestris solis centro australioris, signum — tenendum est. Fiat jam parallaxis horizontalis planetæ a sole = H , quæ in parallaxis investigatione pro lubitu est assumenda, sed ita tamen, ne a vera multum abludat; fiat quoque parallaxis altitudinis planetæ a sole $CL = P$;

atque erit $P = H. \cos. (C \mp E)$; vel, si E negligi queat, $P = H. \cos. C$.

S E C T. IV.

Hiscæ præstructis, formulæ jam exhibendæ sunt, ad quas effectus parallaxis circa contactuum momenta supputentur. Sit igitur ecliptica $K \oslash$ (fig. 4.) ad nodum descendantem, vel $K \otimes$ (fig. 5.) ad nodum ascendentem; NE semita planetæ apparens e centro telluris visa; C commune centrum projecturæ telluris et solis (§ I. et II.); MR meridianus cœlestis; CD latitudo planetæ momento conjunctionis quoad eclipticam; L locus quicunque datus in disco telluris soli obverso; atque patebit ex supra allatis, quod sit in fig. 4. ang. $PCL = Q$; et in fig. 5. ang. $pCL = Q$; nec non recta $CL = P$. Jungantur nunc puncta D et L recta DL ; et fiat $CD = n$; angulus, quem meridianus facit cum ecliptica, nempe in fig. 4. $RC \oslash$ vel in fig. 5. $RC \otimes = b$; nec non ang. $LCD = r$; prodibitque duplicis formæ ipsius r valor, prout semita planetæ ad hunc vel illum nodum, fuerit solis centro vel australior vel borealior: nempe si semita fuerit ad \oslash australior, vel ad \otimes borealior centro solis, erit $r = 90^\circ \mp b - Q$, in qua loco ipsius b sumendum est complementum ejus ad 180° , quoties observatio fuerit antemeridiana. Ast existente semita planetæ ad \oslash borealiore, vel ad \otimes australiore solis centro, erit $r = Q \mp b \pm 90^\circ$, in qua signa superiora in postmeridianis, et inferiora in antemeridianis observationibus adhibenda sunt. Statuatur ulterius $\frac{180^\circ - r}{2} = t$; nec non $\left(\frac{n - P}{n + P}\right) \text{ tang. } t = \text{tang. } x$; atque fiat ang. $CDL = y$; eritque $y = t \pm x$, in qua signum $-$ obtinet locum, quoties $n > P$;

$n > P$; $DL = \frac{P \sin. r}{\sin. y}$. Cumque datur angulus semitæ planetæ cum circulo latitudinis, qui dicatur e , adeo ut sit in fig. 4. ang. $E D I = e$, et in fig. 5. ang. $C D E = e$; dabitur quoque hinc ang. $L D F$ vel ang. $L D E$. Si jam centro L et radio, æquali summæ vel differentiæ semidiametrorum solis et planetæ, qui dicatur m , fiant sectiones, *i. e.* in semita; erit, ob motum planetæ retrogradum, punctum orientalius i locus centri planetæ dum spectari in L immergere incipit vel desinit; punctum vero occidentalius e , pro loco centri planetæ, circa contactus emerfionis, habendum est. Ut autem hi contactus calculo exhibeantur, determinandum erit latus $D i$ vel $D e$ in triangulo jam dato $D L i$ vel $D L e$. Fiat igitur $e + y = u$, in qua circa signa tenendum, quod existente semita sideris ad 8 australiore, vel ad 8 borealiore solis centro; signum — adhibendum erit in observationibus postmeridianis, excepto casu, quo $Q > 90^\circ + b$; signum vero + valet in observationibus antemeridianis, nisi fuerit $Q < 90^\circ - b$. Quoties autem semita planetæ ad hos nodos tenuerit situm oppositum, ordine inverso adhibenda sunt hæc signa: nempe + in postmeridianis, et — in antemeridianis observationibus; nisi dederint istæ $Q > 90^\circ + b$ et hæc $Q < 90^\circ - b$. Posita nunc $\frac{P \sin. r \sin. u}{m \sin. y} = \sin. z$; prodibit $D i = \frac{m}{\sin. u} \sin. (u + z)$, (A); nec non $D e = \frac{m}{\sin. u'} \sin. (u' + z')$, (B); quarum (A) immerfionis, (B) autem emerfionis contactuum supputationibus inservit*.

* Ad hanc methodum, in dissertatione mea supra citata (§ 1.), omnium primo exigebam calculum magni Halley, qui primus

C O R O L. I.

Si centro C et radio $= m$, fiant sectiones in semita I et E ; et si ponatur $P = o$; coincident puncta L , i et e cum C , I et E respective; atque habebitur

nos docuit, parallaxin solis exactissime determinatum iri, per observationes transitus ϕ sub disco \odot (vide Transact. Vol. XXIX. p. 454, &c.); ut sic mihi constaret, quo jure vir hifce, de variis scientiis maxime promeritus, erroris, in designatione locorum commissi, ab aliis atque aliis accusaretur. Calculo itaque subducto ad elementa, a celeberr. Halley adhibita, obtinui, per formulas (A) et (B), pro loco latitudinis borealis 22° , solemque sub medio ϕ transitu sibi verticalem habente, $Di + De = 1716''$, quæ, in tempus conversa, præbet moram apparentem ϕ intra solem $7^h 9'$. Aptato autem calculo ad meridianum oppositum et latitudinem borealem 56° , inveni $Di + De = 1775''{,}7$; unde mora hic $7^h 23' 56''$, quæ cum ista collata, præbet differentiam moræ $= 14' 56''$, paucis duntaxat secundis differentem a calculo Halley, exhibente moram ad Nelsoni portum $15' 10''$ majorem ista ad ostia Gangis. Ast correctis elementis calculi per recentiores tabulas astronom. obtinui, respectu prioris loci, $Di + De = 1316''{,}7$, adeoque moram ibi $5^h 40' 10''$ in posteriori autem loco moram obtineri non potuit, ob Venerem e sole ibi oriente jam egressam. Assumpto itaque in eodem meridiano loco paulo borealiore, latitudinis nempe 60° , prodiit $Di + De = 1370''{,}5$, unde mora ibi $5^h 42' 38''$, duobus solummodo minutis cum dimidio circiter excedens priorem illam moram; omnino ut celeberr. De L' Isle primus deprehendit. Proinde celeberr. Halley rite argumentatus est ex elementis, quæ adhibuit; nec error ei, sed elementis adscribendus est, imprimis vero latitudini ϕ in sole, quæ postea magis quam duplo major deprehensa est: pro cujus diversitate, diversam quoque fore differentiam moræ, Halley ipse haud obscure loc. cit. indicavit. Subductionis vero angulorum $8^\circ 28'$ et $6^\circ 10'$, alterius ab altero factæ, mentionem facere haud meretur; cum inde vix aliquot secundorum error in calculum emanaverit.

$$DI = \frac{m}{\sin. e} \sin. (e \pm c), (C); \text{ atque } DE = \frac{m}{\sin. e} \sin. (e \mp c), (D), \text{ existente } \sin. c = \frac{n. \sin. e}{m}.$$

Ad harum formularum tenorem contactus e centro telluris spectati supputentur. Signa vero æquationum (A), (B), (C) et (D) ita observentur, ut superiora valeant, si planeta, ad ☉ australi, vel ad ☉ boreali latitudine solem trajiciat. Ad latitudinem autem planetæ in sole, in his nodis oppositam, signa inferiora sunt tenenda.

C O R O L. II.

Effectus itaque parallaxis evadet circa immersionem $= \frac{m}{\sin. u'} \sin. (e \pm z) - \frac{m}{\sin. e} \sin. (e \pm c)$, qui in tempus conversus, auferendus, si negativus: si vero positivus fuerit, addendus erit momento observationis, quo habeatur momentum illud ad centrum telluris reductum. Circa emersionem autem erit parallaxis effectus $= \frac{m}{\sin. u'} \sin. (u' \mp z') - \frac{m \sin. (e \mp c)}{\sin. e}$, qui in tempus mutatus, si negativus, addi momento observationis: si verò positivus evadat, eidem demi debet, ut habeatur momentum observatum ad centrum telluris reductum.

C O R O L. III.

Si $n = 0$, i. e. si semita planetæ centrum solis trajiceret; coincidente tunc puncto D cum C (fig. 6.), obtinebitur $Ci = \frac{m}{\sin. s} \sin. (v \pm s)$ pro immersione, atque

atque $Ce = \frac{m}{\sin. v'} \sin. (v' \mp s')$ pro emerfione. In æquationibus autem (C) et (D) (Cor. I.), evanescit nunc c ; quare pro centro telluris relinquitur $CI = CE = m$. Atque hinc erit effectus parallaxis, pro hoc casu, circa immerfionem $\frac{m}{\sin. v} \sin. (v \pm s) - m$; nec non circa emerfionem $\frac{m}{\sin. v'} \sin. (v' \mp s') - m$. Quod signa attinet, superiora circa tam \otimes quam \oslash tenenda sunt, quoties observatio antemeridiana $Q > 270^\circ - b - e$, aut postmeridiana $Q > 270^\circ + b$ non dederit; in his enim casibus signa inferiora valent. Præterea monendum est, me posuisse $\sin. s = \frac{P \sin. v}{m}$; atque $v = e \pm r$, ubi $+$ in observationibus antemeridianis, et $-$ in postmeridianis obtinebit locum, nisi istæ $Q < 90^\circ - b$, et hæc $Q > 90^\circ + b$ dederint. Ceterum pro v , excessus ipsius supra 180° sumendus est, quoties casus $Q > 270^\circ - b - e$, aut $Q > 90^\circ + b$ occurrerit.

S E C T. V.

Exposita sic et ad singulos casus extensa methodo, quæ in hujusmodi disquisitionibus commode adhibeatur; observationes jam sunt adferendæ, quibus in parallaxi solis investiganda usus sum. Ecce igitur in hunc finem sequentem tabellam, in qua per *contact.* 1. immerfionem totalem; per *contact.* 2. emerfionis initium; et per *contact.* 3. emerfionem totalem, designatum volui. Quod longitudines locorum, ad meridianum Parisiense relatas, quæ comparent in secunda columna, attinet; plerasque istarum tales adhibui, quales ab astronomis jam pridem sunt stabilitæ: excepta

cepta longitudine *Bononiensi*, quam clariss. Canterzanus in epistola ad Hieronymum Saladinum, anno 1764 data, ex disquisitione celeberrimi Zanotti, non majorem $35' 53''$ esse evincit. In longitudes autem *Capitis B. Spei*, *Tobolii* et *Selenginski* inquisivit celeberr. Wargentini in *Actis Stockb.* pro anno 1763, unde istas desumsi. Denique quod attinet longitudinem *Pekini*, istam $7^h 35' 50''$ non excedere, celeberr. Rumoufsky in tractatu, quem *investigationem parallaxeos solis* vocat, evincere conatus est.

Nomina locor. et Observatorum		Longitudo.	Latitudo. 1	Latitudo. 2.	Latitudo. 3.
		h m s	h m s	h m s	h m s
Cap. B. Spei.	Mafon	1 4 25 or.		9 39 52	9 57 23
	Dixon			9 39 48	9 57 21
Bononiæ.	Frifius	0 35 53		9 4 56	9 22 59
	Marinus			9 4 58	9 23 0
	Matheucius			9 4 58	9 23 7
	Com. Castilio			9 5 0	
Parisiis.	Le Monnier	0 0 0		8 28 19	8 46 47
	De la Lande			8 28 26	8 46 50
	Clouet			8 28 27	8 46 55
	Baudouin			8 28 27	8 46 46
	Fouchy			8 28 29	8 46 40
	Ferner			8 28 29	8 46 40
	Messier			8 28 29	8 46 37
	De la Caille			8 28 37	8 46 49
	Merville			8 28 40	8 47 4
	Condamine			8 28 42	8 46 49
	Maraldi			8 28 42	8 46 54
	Mayer	0 30 11		8 58 26	9 16 54
	Bliss	0 9 10 oc.		8 19 0	8 37 9
	Short			8 18 50	8 37 28
Göttingæ. Grenovici.	Dollond			8 18 58	8 37 14
	Canton			8 18 58	8 37 21
	Wykstrom	0 56 13 or.	3 33 1	9 23 40	
	Wargentini	1 2 50	3 39 23	9 30 8	9 48 9
Upsaliæ.	Klingenstierna		3 39 29	9 30 11	9 48 8
	Bergman.	1 1 10	3 37 43	9 28 9	9 46 30
	Mallet		3 37 56	9 28 2	9 46 29
	Melander		3 38 2		9 46 29
	Stromer		3 38 5	9 28 0	
Cajaneburgi.	Planman	1 41 30	4 18 5	10 7 59	10 26 22
Torneæ.	Hellant	1 27 39	4 3 59	9 54 8	10 12 22
Tobolii.	Chappe	4 23 45	7 0 30	12 49 23	13 7 42
Selengiski.	Rumoufsky	6 57 5		15 21 36	15 39 42
Pekini.	Dollier	7 35 50	10 10 27	15 59 59	16 17 57

S E C T. VI.

Discimus ex tabella præcedenti ingens extitisse discrimen observationum, ab exercitatissimis astronomis, uno eodemque loco captarum. Etenim Maraldinum momentum contactus interioris a Monnierio 23 secundis differt. Contactus vero exterior a clariss. Messier captus totis 27 secundis, antevertit istum Mervilleanum. Vix minor deprehenditur dissensus observationum contactus immersionis: nam hujus contactus momenta a celeberr. Stromer et Bergman Upsaliæ capta 22 secundis discrepant. Hinc dissensus observationum moræ Veneris intra solem dodrantem minuti primi excedere potest; id quod mora Stromeriana cum Wargentina collata satis ostendit. Tanti autem discriminis causam eo minus petendam esse, ex diversa tuborum longitudine, existimaverim, quo certius constat tubos, a celeberrimis his viris adhibitos, longitudine parum admodum discrepasse; nam differentia longitudinis tubi Maraldini et Monnierii erat solummodo trium pedum. Messier atque Merville usi sunt telescopiis 60 atque 72 pollicum. Quid? quod celeb. Wargentini atque Bergmanni tubus unius duntaxat pedis longitudine excederet tubum Stromerianum. Itaque non ex diversa tuborum longitudine, sed ex alio fonte, nempe ex radiorum refractione, in atmosphæra Veneris facta, imprimis derivanda est enormis ista observationum discrepantia. Ut autem hoc clarius constet, exhibeat arcus L A L (fig. 7.) circa interiorem, et I A I

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circa

circa exteriorem contactum, partem limbi solaris; arcus VTV particulam φ immergentis aut emergentis; nec non cingulum VMMV partem atmosphæaræ φ . Ponatur nunc φ eum obtinuisse situm, ut recta, jungens punctum limbi solis A et oculum spectatoris in O, tangat discum φ in T; quare radii, qui ex A ad O emittuntur, in atmosphæra φ , quam trajiciunt, bis refringuntur, nempe in R. et E; quamobrem observatori in O punctum A videbitur in a . Cumque duplex hæc refractionis competit reliquis quoque punctis huicce A adjacentibus, perspicuum est, circa contactum interiorem exhiberi in limbo solis gibbum quendam luminosum GAG, ast, circa exteriorem, discum solarem deficere figura quadam H α H. Gibbus autem iste erit, circa immersionem, maximus eo ipso momento, quo recta R.E, radii refracti via in atmosphæra φ , tangit φ in T; inde vero decrescit, usque dum recta AO atmosphæram φ tangit, quo momento gibbus iste evanescit, limbi solaris circularis figura restituitur, ac Venus, aliquantum jam intra solis discum demersa, genuina solis luce circumdatur. E contrario gibbus hic, circa emersionem, crescit ab eo momento, quo radius, ex A ad O delatus, atmosphæaræ φ primum occurrit, usque ad id momentum, quo R.E tangit Venerem lumenque gibbi disparet. Hinc mihi valde verosimile videtur, alios posterius illud momentum, quo gibbus disparuit, contactui interiori assignasse; alios iterum id momentum, quo generabatur gibbus, pro immersione totali, vel emersionis initio habuisse; imprimis quia gibbus, φ immergente, tam maximus erat, et ea propter cum lumine limbi solaris facile confundendus ab illis, qui ad gibbosam istius figuram non animadverterint. Venere autem emergente,

gente, gibbus inter generandum minimus erat, quapropter, ceu obscurior reliquo limbo solis, specie emergentis Veneris observatorem fallere potuit. Hujus refractionis phænomena observatores non potuit non æque suspensos tenere de rite capiendo momento egressus totalis: nam eo ipso momento, quo crederes & a sole divulsum iri, spectanda relinquitur, in margine disci solaris, figura anguli cujusdam obtusi $H a H$, qui magis magisque factus acutus, ictu oculi evanuit. Hoc phænomenon Stockholmæ binis, et Upsaliæ singulis observatoribus omnino erat momentaneum; et verosimile mihi quidem occurrit, plures alios idem momentum pro totali egressu habuisse: unde factum est, quod his & diutius in o videbatur, quam talis, qui ad figuram hanc, margini solari superimpositam, non adimadverterunt. Quemadmodum itaque hinc jam patescere existimaverim, palmarium observationum discrimen, variis istis phænomenis, a refractione radiorum in atmosphæra & pendentibus, adscribendum esse, si excipias dissensum, qui in hisce debetur diversitati tuborum aliarumque circumstantiarum; ita quoque speraverim, hæc unumquemque cautum reddere, in capiendis ingressus et egressus momentis, in proximo & transitu. Meo quidem iudicio, pro ingressu totali, capiendum erit momentum, quo filum luminis solaris coalescit, peripheria ejus circulari restituta. At, pro egressus initio, habendum est momentum totalis disparitionis luminis, ad marginem & occidentalem factæ. Egressus vero totalis momentum habeatur plenaria limbi solaris restitutio, quo nempe phænomenon illud nuper descriptum et a refractione pendens evanescit. Vellem insuper, ut id momentum quoque consignaretur,

tur, quo circa contactum exteriorem figura ista angularis in margine solari formatur, et quo circa interiores contactus ibidem gibbus generatur, una cum fascia quadam nigricante, quam nonnulli, ultima vice, ex q̄ limbo, in directionem gibbi, simul exeuntem deprehenderunt. Si vero omnia exacte æstimare non daretur; ad minimum ista phænomena sunt accuratissime consignanda, quæ in capiendis ingressus egressusque momentis conspiciuntur. Alias enim selectionem observationum, si rite instituatur, frustra tentaveris; cum tamen ejusmodi selectio maxime e re foret, in disquisitione parallaxis solis, quæ similiter captis observationibus erit peragenda.

S E C T. VII.

Sed exhibeantur jam parallaxes solis horizontales, quas mihi dabat comparatio observationum utriusque contactus, ad *Caput B. Spei*, et singulorum trium contactuum *Pekini* captarum, cum respondentibus. Valores autem quantitatum prout mihi aut ex observationibus, aut ex *Tab. Astron.* constabant, adhibui sequentes: nempe $D = 67^{\circ} 18' 26''$; $H = 20''{,}6$, supposita parallaxi solis horizontali $8''{,}2$, qua usus sum; $n = 579''{,}6$; $b = 83^{\circ} 51'$; $e = 81^{\circ} 30'$; diametrum solis $= 31' 35'' \frac{1}{2}$; Veneris vero $= 57 \frac{1}{2}$; motumque horarum q̄ in semita apparenti $= 4' 1''$.

Parallax Solis Horizontalis supputata ex
Observationibus peractis.

Nomina Obsery.	Ad Caput B. Spei.		Et Pekini.		
	Cont. 2.	Cont. 3.	Cont. 1.	Cont. 2	Cont. 3.
	"	"	"	"	"
Frifius.	8 ,18	8 ,20	—	8 ,86	8 ,57
Mārinus.	8 ,13	8 ,18	—	8 ,92	8 ,59
Matheucius.	8 ,13	8 ,01	—	8 ,92	8 ,80
Comes Caffalio.	8 ,08	—	—	8 ,98	—
Le Monnier.	8 ,62	8 ,21	—	8 ,20	8 ,60
De La Lande.	8 ,53	8 ,16	—	8 ,36	8 ,65
Clouet.	8 ,51	8 ,09	—	8 ,39	8 ,73
Baudouin.	8 ,51	8 ,23	—	8 ,39	8 ,58
Fouchy.	8 ,49	8 ,29	—	8 ,43	8 ,42
Ferner.	8 ,49	8 ,29	—	8 ,43	8 ,42
Meffier.	8 ,49	8 ,20	—	8 ,43	8 ,32
De La Caille.	8 ,35	8 ,18	—	8 ,62	8 ,63
Merville.	8 ,30	7 ,93	—	8 ,68	8 ,95
Condamine.	8 ,28	8 ,18	—	8 ,77	8 ,63
Maraldi.	8 ,28	8 ,11	—	8 ,77	8 ,71
Mayer.	8 ,24	7 ,80	—	8 ,88	9 ,22
Blifs.	8 ,46	8 ,43	—	8 ,48	8 ,26
Short.	8 ,58	8 ,07	—	8 ,24	8 ,82
Dollond.	8 ,49	8 ,33	—	8 ,45	8 ,39
Canton.	8 ,49	8 ,22	—	8 ,45	8 ,59
Wykftrom.	8 ,39	—	8 ,52	8 ,63	—
Wargentın.	8 ,20	8 ,29	8 ,06	9 ,20	8 ,51
Klingentfiern.	8 ,15	8 ,31	8 ,48	9 ,35	8 ,48
Bergman.	8 ,49	8 ,24	8 ,06	8 ,69	8 ,64
Mallet.	8 ,58	8 ,26	8 ,96	8 ,10	8 ,61
Melander.	—	8 ,26	9 ,36	—	8 ,61
Stromer.	8 ,62	—	9 ,56	8 ,00	—
Planman.	8 ,38	8 ,10	8 ,64	8 ,83	9 ,20
Hellant.	8 ,28	8 ,17	8 ,28	9 ,26	9 ,07
Chappe.	8 ,43	8 ,12	8 ,39	—	—
Rumoufski.	8 ,26	8 ,12	—	—	—
Dollier.	8 ,46	8 ,37	—	—	—
Per. Medium.	8 ,38	8 ,18	8 ,63	8 ,62	8 ,65
					Sumto.

Sumto nunc medio horum mediorum, evadit folis parallaxis $8''$,49. Si autem rejiciantur parallaxes, quæ prodeunt ex Pekinensium observationum comparatione, ob longitudinem *Pekini* nondum certo stabilitam; relinquitur folis parallaxis $8''$,28, cuj medij, ex observationibus ad *Caput B. Spei* factis, deductum.

S E C T. VIII.

Ut autem constaret, quo jure celeberr. Pingré in tractatu, quem *Parallaxe de Soleil* vocat, dubias reddere conatus est observationes ad *Caput B. Spei* peractas, instituendas esse duxi plures comparationes, quarum tamen istæ, observationibus moræ & intra solem superstructæ, ob effectuum parallaxis exiguum, observationum vero nimiam discrepantiam, hic adferri non merentur. Itaque eæ comparationes exhibendæ restant, quæ nituntur mediis utriusque contactus observationibus et *Parisiis* et *Bononiæ* captis, in quem finem, ecce sequentem tabellam :

Nomina Observ.	Parisiis.		Bononiæ.	
	Cont. 2.	Cont. 3.	Cont. 2.	Cont. 3.
	"	"	"	"
Rumoufsky.	8 ,00	7 ,98	8 ,44	8 ,11
Chappe.	8 ,83	7 ,88	9 ,02	8 ,12
Hellant.	7 ,81	8 ,00	8 ,63	8 ,25
Planman.	8 ,20	7 ,75	9 ,00	8 ,08
Strömer.	9 ,52		10 ,25	
Mallet.	9 ,33	8 ,37	10 ,10	8 ,66
Melander.		8 ,37		8 ,66
Bergman.	8 ,67	8 ,29	9 ,65	8 ,59
Wargentín.	7 ,13	8 ,69	8 ,43	8 ,75
Klingenstierna.	6 ,83	8 ,78	8 ,20	8 ,83
Per Medium.	8 ,22	8 ,23	9 ,08	8 ,49

Hinc

Hinc iterum per medium habetur solis parallaxis $8'',49$. Rejecta autem columna tertia, ceu maxime discrepante, dabunt reliquæ solis parallaxim $8',30$, quæ cum parum admodum abludat ab ista $8',28$, quam maximi momenti observationes præbebant (§ VII.); vi novissimi transitus Veneris, parallaxis solis horizontalis quam proxime statuenda est $8'',28$, falvis differentiis meridianorum, quas adhibui. De cætero, sunt mihi rationes, quæ parallaxin potius minuendam, quam augendam esse suadent: sed mitto has, donec proximus Veneris transitus sub disco solis, modo ex voto succedant observationes, in rem subtilissimam exactius inquirendi ansam nobis subministraverit.

XVII. *A short Account of the Manner of inoculating the Small Pox, on the Coast of Barbary, and at Bengal, in the East Indies, extracted from a Memoir written in Dutch, by the Reverend Mr. Chais, at the Hague : by M. Maty, M. D. S. R. S.*

Read April
14, 1768.

HAVING long thought that the Arabs, who, about the middle of the sixth century, were the first who wrote upon the small-pox, were likewise the first inventors of the method to prevent the fatal consequences of that cruel disorder, I was very desirous to get what informations I could concerning the introduction of inoculation in Africa, and in the East Indies.

About twenty years ago, Cassen Aga, a Tripolitan ambassador at London, informed the people about him, that inoculation was universally practised, as well at his court, as at Tunis and Algiers; but that no certain account could be given, either of the introducers of the method, or of the place from whence it took its rise.

One of the chief ministers of state in Holland was so good, on this information, and at my desire, to send a few queries on that subject, drawn up by myself, to a gentleman, who, for several years, has resided with a public character at Algiers. The following is a summary of his answers to my queries.

“ The small-pox is, as well as in Holland, a contagious distemper at Algiers, Tunis and Tripoli,
“ and

“ and fully as destructive. In order to avoid the
 “ bad consequences of the natural disorder, many
 “ people have recourse to inoculation, which there
 “ is performed in a very different manner from what
 “ is used in our country. The person, who intends
 “ to be inoculated, having found out a house,
 “ where the small-pox is, and is of a good sort;
 “ goes to the bed of the sick person, if he is old
 “ enough, or, if a child, to one of his relations; and
 “ speaks to him in the following manner; *I am come*
 “ *here to buy the small-pox*: the answer is, *buy if you*
 “ *please*. A sum of money is accordingly given, and
 “ one, three, or five pustules (for the number must
 “ always be an odd one, not exceeding five), extract-
 “ ed whole, and full of matter. These are immedi-
 “ ately rubbed upon the skin of the hand, between
 “ the thumb and fore-finger. This is sufficient to
 “ communicate the infection; and as soon as it be-
 “ gins to take effect, the inoculated patient is put to
 “ bed, carefully covered with red blankets; and
 “ heating medicines are given him with some honey
 “ of roses. He is allowed goat’s broth for his nou-
 “ rishment, and for his drink an infusion of some
 “ herbs; notwithstanding this treatment, it seldom
 “ happens that the small pox procured in this man-
 “ ner has any bad consequences; and almost never
 “ that any body dies of it; but hitherto the propor-
 “ tion of the mortality in the natural, to that in the
 “ artificial way, has not been ascertained. Lastly,
 “ though the time when this practice was introdu-
 “ ced in Africa be unknown, yet it is there very old;
 “ and the Arabs are generally thought to have been
 “ the inventors of it.”

From this account it plainly appears; 1. that in Africa the operation is performed as it is in Wales, by the rubbing in of the matter, and that this is done to prevent the fatal consequences too often following the natural infection; 2. that this inoculation is generally successful, notwithstanding the heat of the climate, and the bad management of the patients; and 3. that the origin of it is very ancient, and ascribed to the Arabs.

Before I had received these informations from Algiers, I had engaged some friends settled in three different parts of the East Indies, to procure me some accounts from thence, upon the same subject. I, at last, received an answer from one of them, who resides at Patna, in the province of Behaar, 180 leagues from Bengal.

“ I have sent for several physicians, to be informed of the things you seem desirous to know about inoculation; the practice is hitherto not used in this province; but having met with a Bengalian doctor, he gave me the following account.

“ Though the first introduction of the operation at Bengal is now unknown, it has been in use in that country for a very long time, and is performed in two different ways.

“ For the first, some of the variolous matter of a good kind having been gathered is kept for use. When a child is to be inoculated, the skin between some of the fingers is pricked by means of two small needles joined to one another. After having rubbed in a little of the matter upon the spot, a circle is made by means of several punctures, of the bigness of a common pustule; and matter is again rubbed over it. The wound is then dressed with

“ lint; a fever ensues, and after some days, the
 “ eruption, which if the fever has been strong is ob-
 “ served not to be very copious. To excite the fe-
 “ ver, the patient is made to bathe in a tub of
 “ water.

“ As this way of managing the operation is very
 “ painful, a more easy one has been invented for peo-
 “ ple of quality and substance. A little of the matter
 “ is mixed with sugar, and swallowed by the child in
 “ any sweet and pleasant liquid. The same effect
 “ is produced, but the first method is thought to be
 “ the best.”

The writer of this letter ought certainly to have been more particular in his inquiries; he might have asked whether any preparation previous to inoculation is used, and of what kind; what treatment the patients undergo after the operation; and lastly, how far the event warrants the goodness of the method. It appears however, from what he says, that the people of Bengal have for a long while had recourse to inoculation, in order to avoid the dreadful consequences of the natural distemper in their country; and it is to be wished that farther inquiries be made, both there and elsewhere, about a subject which so nearly concerns the good of mankind.

XVIII. CROTON Spicatum, *nova Plantæ Species ex America, quam Descriptione ex iconè illustravit Petrus Jonas Bergius, M. D. Hist. Nat. et Pharm. Profess. Stockh. R. Colleg. Med. Assessor, Reg. Acad. Scient. Stock. Membr.*

Read April, 21,
1768.

SCIENTIÆ rei herbariæ ulteriori perfectioni nihil perinde conducere arbitror, atque idoneas plantarum novarum minusve cognitarum descriptiones, præsertim ubi bonæ etiam accesserint icones. Ut enim in omnibus aliis scientiis magnopere expedit, quarumcunque rerum eo pertinentium ignorantiam vel incertitudinem, quoad fieri poterit, tolli, ita valde quoque opportunum scientiæ contingit botanicæ, quotiescunque novæ rarissimæque plantæ in lucem proferuntur, et quidem ita proferuntur, ut quælibet partes earum essentielles, quoad liceat, et illucide adumbrentur. Certe quidem hoc generatim de omnibus valet plantis, ad quodeunque demum stirpem pertinerint genus; nulla enim unquam detecta fuit species, quin attentione fuerit dignissima; tamen solent speciatim illæ ipsæ noviter detectæ species impensiori excipi eruditorum attentione et applausu, quæ ad genus quoddam pertinent, in quo una pluresve species virtute usuque aliquo singulari, vel medico vel œconomico, dudum inclaruerunt, quippe quoniam suspicari fas sit, in cunctis speciebus affinis quodammodo consonas, saltem non multum alienas, inesse vires.

Hocce respectu operæ profecto pretium fuerit, rarissimam quandam sistere speciem, ad amplum illud pertinentem stirpium genus, quod utraque potissimum India profert, quodque botanicis *Croton* audit. Etenim non una duntaxat, sed plures species, sub unius generis vexillo comprehensæ, ob eximium in medicina aut œconomia usum multo dudum nomine sunt, quod quidem satis ii norunt, qui ex merito æstimare didicerunt *Croton Castarillam*, *Croton febriferum*, *Croton Tiglium*, *Croton tinctorium*, atque *Croton aromaticum*. Hinc ubi nova eidem huic generi accedit species, adeo non id a botanicis ferri poterit indifferenter, ut potius non possint non primum eo advertere animum oculosque. Et vero etiam nondum quidquam de illa novæ stirpis virtute constet, tamen id movere poterit neminem, qui, uti par est, cogitaverit, præcedere semper ante oportere primam rei cujusque notionem, quam vel minima de qualitatibus ipsius oboriatur quæstio.

Hicce jam pensitatis, propius me ad ea, quæ de planta mea observanda habui, conféro: Et quidem adeo illam novam dicebam, quod cum manca duntaxat ac admodum imperfecta mentione ad notitiam botanicorum pervenit, adeoque nec ab ullo adhuc systematicorum in album stirpium cognitarum est relata. Certe LœFLINGIO nostrati in America peregrinanti, illam ipsam visam fuisse, haud obscure ex itinerario ejus patet, ubi tamen non nisi breviter et quasi per transennam unam alteramque ejus tradit notam, quanquam quidem fusius sine dubio reliqua persecutus fuisset, nisi fato occubuisset præmature. Ceterum nec multum refragabor, si quis contendit, arborem illam mali folio, quam profert doctiss. H. SLOANE, *Hist. Jam. tom. ii. p. 30. tab. 174. f. 1.*
Croton

Croton esse spicatum, tametsi non negem, unam alteramque discrepantiam cerni posse evidentiorē, quod fortasse maximam partem pictori tribuendum; interim tamen suadent convenientiam imprimis spicæ terminales, ut reliqua taceam.

Icon adjecta fruticis ramulum exactissime refert magnitudine naturali, lectum ante biennium Havanae a chirurgo N. RUDOLPH.

T A B. VII.

CROTON.

CROTON (*spicatum*) foliis ovatis glabris, ramis nudis, florum racemis spicatis terminalibus. Croton foliis ovatis, floribus spicatis, stylis multifidis depresso patentibus, frutescens. LOEFFL. *It. p. 234. n. 50.*

Mali folio arbor; artemisiæ odore, flore pentapetalo spicato. SLOAN. *Hist. Jam. ii. p. 30. t. 174. f. 1.* vix bona. *Cat. Jam. 139.* RAJ. *dendr. 17.* Habitat Havanae in America.

DESCR. *Caulis* fruticosus, ramosus. *Rami* subterminati, cinerei, subrugosi, erecti, nudi, cicatricibus obsoletis, ramulosi. *Ramuli* confimiles; tenelli, glabri, sulcati. *Folia* solummodo in ramis tenellis, alterna, ovato oblonga, integerrima, basi rotundata, apice sublanceolata, obtusa, firmisscula, utrinque glabra, petiolo instar nervi longitudinalis discum folii subtus percurrente, nervis obliquis tenuioribus nervosa, bipollicaria vel paulo ultra, frequentia, petiolata, patentia. *Flores* MASCULI racemoso-spicati, pedicellati. *Racemi* terminales, pyramidales, compactiusculi, solitarii, pedunculati. *Bractæ* lineares, obtusæ,



CROTON spicatum.

sæ, glabræ, ad basin pedunculi racemi sitæ. *Pedicelli*
 subpubescentes. CALYX. *Perianthium* decaphylli
 sæpe ultra, imbricatum: squammis subæqualibus,
 ovatis, obtusiusculis, extus glabriusculis, intus hirsu-
 tis: hirsutie albida. COROLLA nulla. STAMINA.
Filamenta xii, sæpe ultra, subulata, inferne hirsuta,
 erecta, longitudine calycis. *Antheræ* subrotundæ,
 compressæ, subtetragonæ, erectæ. *Receptaculum* sta-
 minum hirsutum. FLORES FEMINEI solitarii, ad basin
 pedunculi racemi masculini intra bracteam fiti pe-
 dunculati, erecti. CALYX. *Perianthium* pentaphyl-
 lum, infernum: foliolis lineari-lanceolatis, acutiusculis,
 erectis, subæqualibus, minutissime punctatis, subca-
 biusculis. PISTILLUM. *Germen* subrotundum, com-
 pressum, subhirsutum. *Styli* tres, longitudine calycis,
 infima basi cohærentes; singuli ad medium sexfidi:
 laciniis ex uno puncto prodeuntibus, subulatis; unde
 ramosi apparent. *Stigmata* xviii, obtusa.

OBS. A Croto glabello LINN. caute distinguendum;
 etenim in illo rami foliosi, flores racemoso-paniculati,
 axillares.

Read April 28, 1768.

XIX. Observations on the Barometer and Thermometer, and Account of the whole Rain in every Month of the Year 1767, taken at the Royal Hospital near Plymouth: By William Farr, M. D. Transmitted to William Watson, M. D. F. R. S.

Month. 1767.	Barometer.	Therm.	Winds from what Quarter highest.	Rain.	General Account of the Weather.
January	Highest state 30° 20' Lowest D° 28° 67'	D° 53 D° 24	E. by N. } S. E. } S. }	4 630	First part of the month clear; and frost; remainder much rain and stormy weather. January 1st, remarkable gulf of wind E. by N.
February	H. 30° 05' L. 29° 04'	H. 53 L. 48	S. S. W. } S. W. }	7.000	Constant rains, only three fair days during the whole month. High winds on the 11th, 19th, and 26th.
March	H. 30° 23' L. 29° 28'	H. 55 L. 44	N. W. } W. by N. }	4.270	Sky serene and clear the greatest part of the month; towards the middle and latter end very heavy rains.
April	H. 30° 41' L. 29° 05'	H. 55 L. 45	S. E. } S. S. E. } S. }	2.250	Rain from the 20th to the 27th. Storm 21st S. E. the rest of the month very fine weather. Wind mostly E. and S. E. and sometimes N.
May	H. 30° 39' L. 29° 25'	H. 59 L. 50	W. } N. } N. N. W. }	2.860	First part of the month very fine weather; middle and end showery, with hail storms. Air generally sharp through the whole.
June	H. 30° 32' L. 29° 44'	H. 64 L. 53	E. by S. } E. by N. }	.882	The 8th the first warm day this year; from thence to the 17th very fine weather, and in general through the month; though the air was often sharp. No high Winds this month.

July

1767. Month.	Barometer.	Therm.	Winds from what Quarter highest.	Rain.	General Account of Weather.
July	Highest state 29 98 Lowest D° 29 11	H. 65 L. 60	W. by N. } W. by S. } 3 S. E. } S. W. } 2'''	5.560	Almost constant rains during the month; only five fair days.
August	H. 30 21 L. 29 50	H. 69 L. 61	N. N. W. } S. W. } 2'''	3.135	First part of the month very fine weather; calm throughout, except from the 14th to the 16th; towards the latter end sultry; 29th, thunder-storm.
September	H. 30 33 L. 29 49	H. 67 L. 60	N. W. 2''' S. 2''	2.531	First part of the month constant rains; from the 18th to the latter end remarkably fine weather, and quite serene.
October	H. 30 23 L. 29 15	H. 61 L. 48	S. W. } W. } 3 S. W. } S. E. } 2'''	3.325	From the 8th to the 15th, weather clear and serene, with sharp frosty mornings; rest of the month rain, or hazy weather.
November	H. 30 37 L. 29 04	H. 10th 60 L. 42	S. W. 10th } S. S. W. 13th } 4 W. by S. 2'''	3.506	Rain frequent till the 21st; from the 8th to the 16th very squally weather; from the 21st to the end of the month fair.
December	H. 30 54 L. 29 02	H. 53 L. 28	E. by S. 3 N. E. 2'''	1.850	Beginning and latter end frosty weather, with little wind; middle, squally with showers.

Total 41.799, or 41 $\frac{3}{4}$ inches of rain nearly.

For the more particular account of the weather, during the late frost, vide the subsequent pages.

State of the Barometer, Thermometer, Wind, and Rain, from *December 20,*
1767, to *January 21,* 1768.

1767. Day. December.	Bar.	Ther. within doors.	Ther. with- out.	Wind.	Rain	Weather, with miscellaneous remarks.
20 9 a. m.	29 2	50		S. E.	2'''	Fair
11 p. m.	29 27	50		S. E.	2''	D°
21 9 a. m.	29 56	50		E. b. S.	2'''	Clear
11 p. m.	29 70	41		E.	2''	D°. N. B. Frost set in this evening
22 9 a. m.	29 80	40		E. b. N.	2''	Frost air very sharp
11 p. m.	29 86	39		N. E.	2'''	D°
23 9 a. m.	29 90	39		D°	2'''	D°
11 p. m.	29 93	38		D°	2'''	D°
24 9 a. m.	30 1	35		D°	2'''	D°
11 p. m.	30 0	35	30	D°	2''	Cloudy, with frost
25 9 a. m.	29 93	35	30	N. N. E.	1	Clear and ferene
11 p. m.	29 75	33	28	D°	2	Cloudy
26 9 a. m.	29 66	33		D°	2'	Sleet at times
11 p. m.	29 60	31	27	N.	1	Clear
27 9 a. m.	29 44	30		N.	0*	Clear and ferene in the morning
11 p. m.	29 41	29		N.	0	Snow since the afternoon
28 9 a. m.	29 44	29		N.	0	Clear; snow lies on the ground about 4 inches deep
11 p. m.	29 55	30		N.	1	Clear
29 9 a. m.	29 77	31		N.	1	Clear and ferene
11 p. m.	29 74	33		S. E.	1	Cloudy; in the night showers
30 9 a. m.	29 76	34		D°	1	180 Clear and frost
11 p. m.	29 74	36		D°	1	D°
31 9 a. m.	29 80	35		D°	2	Fair and frost; towards noon thaw
11 p. m.	29 41	36		D°	2'''	220 Rain with sleet; and in the night snow
1768. January.						
1 9 a. m.	29 0	36		D°	2'''	350 Rain, heavy at times, through the forenoon
11 p. m.	29 0	34		N. E.	2'''	Clear and frost
2 9 a. m.	29 60	33		N.	2''	D°
11 p. m.	30 0	33		N.	2	D°
3 9 a. m.	30 10	29	20	N.	2	D° [N. B. 20 was the lowest state of the therm. ob-
11 p. m.	30 3	30	21	D°	2	erved in these parts, without doors.

1768. Day. January.	Bar.	Ther. within doors.	Ther. with- out.	Wind.	Rain	Weather, with miscellaneous remarks.
4 9 a.m.	30 2	30	20	N.	2''	Fair and frost
11 p.m.	29 92	30	22	D°	2'	Cloudy, with D°
5 9 a.m.	29 90	30		D°	2'	Clear and frost
11 p.m.	29 80	30		D°	2'	Cloudy
6 9 a.m.	29 66	32	30	D°	2'	Snow
11 p.m.	29 60	32		N. E.	2	Hail and sleet, with frost
7 9 a.m.	29 54	32		F.	2	Cloudy
11 p.m.	29 49	34		E. b. S.	2	D°
8 9 a.m.	29 26	35		S. E.	2''	Rain constant, with thaw through the day
11 p.m.	29 20	35		D°	2''	D°
9 9 a.m.	29 10	36		D°	2''	Rain at times
11 p.m.	29 14	36		E.	2''	Rain constant
10 9 a.m.	29 34	37		N. W.	2''	Snow, with rain at times
11 p.m.	29 56	37		N. N. W.	2''	Cloudy
11 9 a.m.	29 60	36		D°	1	Fair and frost
11 p.m.	29 68	36		W. b. N.	1	Thaw, with snow; now frost again
12 9 a.m.	29 66	36		S. E.	2	Rain from 11 a.m. constant through the day
11 p.m.	29 50	39		D°	2	D°
13 9 a.m.	29 47	48		S.	2	Cloudy
11 p.m.	29 36	48		S. W.	2''	Rain since 2 p.m.
14 9 a.m.	29 30	48		D°	2''	Cloudy, with rain
11 p.m.	29 16	49		D°	2	Cloudy
15 9 a.m.	29 25	48		W.	2	Rain heavy till noon
11 p.m.	29 50	47		W. b. S.	1	Fair
16 9 a.m.	29 80	46		D°	0	Fog in the morning; afterwards clear and serene
11 p.m.	29 87	45		S. W.	1	Clear
17 9 a.m.	29 87	45		E.	2	Close and cloudy, with small rain
11 p.m.	29 78	45		S. W.	1	Cloudy
18 9 a.m.	29 46	45		W. b. S.	1	Rain heavy till the afternoon
11 p.m.	29 65	44		N. W.	1	Cloudy
19 9 a.m.	29 67	44		D°	1	Clear and serene
11 p.m.	29 56	44		D°	2''	Rain since 4 p.m.
20 9 a.m.	29 60	44		N. N. W.	4	Fair with flying clouds. N. B. Hurricane from
11 p.m.	29 75	44		D°	2	4 to 8 a. m.; when the Fame Man of War

was driven from her mooring in Hambaze; together with the steer-hulk on the rocks, by Saint Nicholas Island.

N. B. The thermometer within doors is kept in an airy open stair case, and not affected by any fire in the House; that without was in a shed under the North wall.

N. B. From the 2d to the 7th of February was a frost, and since that time incessant rain to March 1.

Received April 20, 1768.

XX. *An Account of Inoculation in Arabia, in a Letter from Dr. Patrick Russell, Physician, at Aleppo, to Alexander Russell, M. D. F. R. S. preceded by a Letter from Dr. Al. Russell, to the Earl of Morton. P. R. S.*

My Lord,

Read May 5. 1768. **T**HE inclosed account of inoculation

in the East, I have just received from my brother at Aleppo, and though nothing farther seems wanting in this country to remove prejudices against that practice, yet I thought its being made public might be of some use to other European nations, where such prejudices still prevail; and as a matter of curiosity, would not be unacceptable to the Royal Society. I have therefore taken the liberty to trouble your Lordship with it for that purpose.

Just before my leaving Aleppo, I did hear that it was practised amongst some of the Bedouins there, and went by the name of buying the small pox; but being then much engaged with other business, it quite escaped my memory, and indeed my information was so slight, that I did not think it right to mention it in my Natural History of Aleppo.

I shall

I shall only add, that my brother has been more prolix in the narrative than perhaps was necessary, had the facts come within his own knowledge; but so far as depended upon the intelligence of others, he thought it best to explain the foundation of his own belief.

I have the honour to be,

My Lord,

Your Lordship's

most obedient servant,

Walbrook, April 18,
1768.

Alexander Ruffell.

Dear

Dear Brother,

FROM the manner in which inoculation is mentioned in the Natural History of Aleppo, I suspect the circumstance of it's being a common practice among the Arabs must have escaped you. I myself was ignorant of it for several years after you left this country, and a mere accident brought it at last to my knowledge. About nine or ten years ago, while on a visit at a Turkish Harem, a lady happened to express much anxiety for an only child, who had not yet had the small pox; the distemper at that time being frequent in the city. None of the ladies in the company had ever heard of inoculation; so that, having once mentioned it, I found myself obliged to enter into a detail of the operation, and of the peculiar advantages attending it. Among the female servants in the chamber was an old Bedouin, who having heard me with great attention, assured the ladies, that my account was upon the whole a just one, only that I did not seem so well to understand the way of performing the operation, which she asserted should be done not with a lancet, but with a needle; she herself had received the disease in that manner, when a child; had in her time inoculated many; adding moreover, that the practice was well known to the Arabs, and that they termed it buying the small pox.

In consequence of this hint, I set about the procuring more particular information from [the Arabs of this place; and the result of my enquiry was, that the practice of inoculation had been of long standing among them. They indeed did not pretend to assign any period to its origin; but those of seventy years old and upwards remembered to have heard it spoken of as a common custom of their ancestors, and made little doubt of its being of as ancient a date as the disease itself. Their manner of operating is, to make several punctures in some fleshy part, with a needle imbued in variolous matter, taken from a favourable kind of pock. They use no preparation of the body; and the disease communicated in this way being, as they aver, always slight, they give themselves little or no trouble about the child in the subsequent stages of the distemper.

This method of procuring the disease is termed, buying the small pox, on the following account. The child to be inoculated carries a few raisins, dates, sugar plumbs, or such like, and shewing them to the child from whom the matter is to be taken, asks how many pocks he will give in exchange. The bargain being made, they proceed to the operation. When the parties are too young to speak for themselves, the bargain is made by the mothers. This ceremony, which is still practised, points out a reason for the name given to inoculation by the Arabs; but, by what I could learn among the women, it is not regarded as indispensably necessary to the success of the operation, and is in fact often omitted.

The Bedouins at this place, who are employed in the service of the Harems, more rarely have recourse to inoculation, their children being often brought up in company with those of the Turks, by whom, as you justly observe, the practice is not admitted. But the Bedouins, less connected with the Turks, who dwell within the city; those who live in tents without the city walls, and the Arabs of the adjacent desert under the Emir, do commonly inoculate their children.

It being highly probable that a practice, which was so common in these parts, might be known also to the more Eastern Arabs, I applied for information to several Turkish merchants of Bagdat and Mousul, who occasionally reside a few months in the year at Aleppo. By those I was assured, that inoculation was not only common in both the cities first mentioned, but also at Bassora; and that at Mousul particularly, when the small pox first appeared in any district of the city, it was a custom sometimes to give notice by a public crier, in order that such as were inclined might take the opportunity to have their children inoculated.

I enquired at the same time of the Bagdat merchants, whether the Arabs, who dwell on the banks of the river between that city and Bassora, used the same method of propagating the small pox. They told me, they believed it to be common also among those Arabs; though (with an ingenuity not usual in this country) they owned they had never thought of enquiring about the matter, and might therefore perhaps be mistaken. But I afterwards had

an opportunity of being better informed by the Arabs; who come hither with the Eastern caravans; from whose accounts it would appear, that inoculation has from time immemorial been a practice among the different Arab tribes with which they were conversant; comprehending, besides those in the numerous encampments on the banks of the Euphrates, and the Tigris below Bagdat, other tribes in the vicinity of Bassora, and in the desert.

For these several years past, very few slaves have been brought from Georgia. From what I could collect among those already here, who remember any thing of their own country, inoculation was well known there: I have seen several old Georgian women, who had been inoculated, when children, in their fathers houses.

In Armenia, the Turkoman tribes, as well as the Armenian Christians, have practised inoculation since the memory of man; but, like the Arabs, are able to give no account of its first introduction among them.

To what extent inoculation reaches in the Gourdeen mountains, I do not know with any certainty: it is practised by the Gourdeens in the Mountains of Bylan, and Kittis; and, I have reason to think, extends much further.

At Damascus, and all along the coast of Syria and Palestine, inoculation has been long known. In the Castravan mountains it is adopted by the Drusi, as well as the Christians.

Whether the Arabs of the desert, to the South of Damascus, are acquainted with this manner of com-

municating the small pox, I have not hitherto been able to learn; but a native of Mecca, whom I had occasion to converse with this summer, assured me, that he himself had been inoculated in that city.

It has already been mentioned, that the Turks at Bagdat and Mousul make no scruple to inoculate their children. I have seen also some Turkish strangers here, who had been inoculated at Erzeroon. Hence it is probable that the Turks, in other parts of the Ottoman empire, do not merely, as fatalists, reject inoculation; but that other considerations, which have influence in countries where fatalists are ridiculed or anathematized, concur likewise in Turkey, to oppose the reception of a practice so beneficial to mankind. The child of a Bashaw here, was by my advice inoculated about eight years ago; but that is the only instance I have known among the Turks at Aleppo.

The Jews at this place absolutely reject inoculation; partly from scruples of a religious kind, and partly from the distrust of its success. At Bagdat, Bassora, and in Palestine, having acquired a more favourable opinion of an operation which they see so often performed with success, they have got the better of other scruples, and join in the practice with their neighbours.

I have several times conversed on this subject with the Musulmans here, as also with some of the Rabbis; but the theology of both was too abstruse for me: their arguments, so far as I was able to comprehend them, seemed to be no less cogent against all surgical operations, which were attended with any

any degree of danger to life, than against inoculation.

In the different countries above-mentioned, inoculation is performed nearly in the same manner. The Arabs affirmed, that the punctures might be made indifferently in any fleshy part: those I have had occasion to examine, have all (a very few excepted) had the mark between the thumb and the fore-finger.

Some of the Georgians had been inoculated in the same part, but most of them on the fore-arm. Of the Armenians some had been inoculated in both thighs; but the greatest part (like the Arabs) bore the marks upon the hand. Some of the Georgian women remembered, that rags of a red colour were chosen in preference for the binding up the arm, a circumstance of which I have been able to discover no trace among the Arabs.

Buying the small pox, is likewise the name universally applied to the method of procuring the disease. There are, it is true, other terms made use of both in the Arabic and Turkish Languages; and at this place, it is principally known to the Christians by the name of inoculation.

From the sameness of the name, as well as from the little diversity observable in the manner of performing the operation, it is probable the practice of inoculation in these countries was originally derived from the same source: and that it is of considerable antiquity, can hardly be doubted, if we consider the large extent of country over which it is found to have spread, and the obstacles it must have met

with in a progress through various nations, of which some are separated by polity as well as religion, while others, peculiarly tenacious of their own customs, are little disposed to admit those of strangers.

That no mention is made of inoculation by Rhazes, Avicenna, or any other of the ancient Arabian medical writers known in Europe, is, I believe, in general supposed; and I am assured by the native physicians here, that nothing is to be found regarding it, in any of a more modern date. Some learned Turkish friends here, some time ago were prevailed on at my request to make enquiry, but have not hitherto been able to discover any thing concerning inoculation; although they searched not only the medical writers, but also the historians, and some of the poets.

It appears from accounts communicated to the Royal Society, in the year 1723, by Doctor Williams and Mr. Wright, that inoculation had been known in certain parts of Wales so far back as the last century; and it is remarkable, that it there bore the same name, by which it is most generally known to the Arabs. I think it has also been discovered to be an ancient practice among the vulgar in different parts of the continent.

If inoculation was really known so long ago in Europe, and the accounts of it till within these fifty or sixty years are found to be merely traditional, the silence of the Arabian writers, on a practice which probably was never adopted by their physicians, is the less to be wondered at. What may perhaps appear more strange, is, that after the year 1720, though the

the curiosity of the public has, at different times, been excited by the controversies relating to inoculation, the state of that practice in Syria, where there were so many European settlements, should have remained unknown both in England and in France, which probably was the case, as the advocates for inoculation have made no reference to it.

Whether before the account transmitted by Pylarini to the Royal Society, inoculation had not been mentioned by any of the travellers who had visited these countries, I do not presume to determine. In the books I have had occasion to peruse, there is nothing to be found on the subject. Among the travellers the most likely to have mentioned it was Rauwolf: yet, however rational it may be to think that a practice of such a kind, had it then prevailed, could hardly have escaped the notice of so diligent an observer, it would be rash to infer from his silence that it was not known to the Arabs in the sixteenth century. The justly celebrated French botanist is equally silent, though in the beginning of the present century he visited several places where inoculation was undoubtedly at that time both known and practised.

Having related in what manner I came to learn inoculation was known to the Arabs, I can arrogate no merit in the discovery; nor would I be thought to insinuate any reflection on the accuracy of the indefatigable M. Tournefort, to whose labours the curious stand so much indebted. Customs the most common, in distant countries, are often of all others
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the least apt to attract the observation of travellers, who, engaged in other pursuit, must be indebted to accident for the knowledge of such things, as the natives seldom talk of, from the belief that they are known to all the world. This consideration may, in some measure, account for inoculation having been over-looked by those who have transiently passed through these countries; and is all we can offer as an apology, for the having remained so long unacquainted with a fact in medical history, in a situation where we both had so many opportunities of information. I am,

Dear brother,

Most affectionately yours,

Aleppo, Nov. 26,

1767.

P. Ruffel.

Received May 5, 1768.

XXI. *Part of a Letter from Dr. Wolfe, at Warfaw, to Henry Baker, F. R. S. Communicated by Mr. Baker.*

Warfaw, April 15, 1768.

Read May 12, 1768. **O**UR winter has been very long and very severe. The frost was at a medium, from the middle of December to the middle of March, at nine degrees of Reaumur's thermometer below the freezing point. Three times it was from 19 to 20 degrees, and very seldom ascended to the freezing point. Last year the frost was at 24 degrees, but of no duration. In the year 1740 it was at 26 degrees, which, perhaps, is the lowest it has been remembered in this country. This year we have had little or no snow, which made the frost exceedingly more sensible. The barometer is here, at a medium, at $28 \frac{1}{3}$ inches Rhinland measure: great storms sink it two inches lower; and very dry weather raises it $1 \frac{1}{3}$ inch higher. The declination of the needle at this place is eleven degrees and half to the west.

XXII. *Extract*

XXII. *Extract of a Letter from Mr. Peter Wargentin, Secretary of the Royal Academy of Sciences at Stockholm, and F. R. S. dated February 23, 1768, to the Reverend Nevil Maskelyne, B. D. F. R. S. Astronomer Royal.*

Read May 12, 1768. **A**S it is related in the public newspapers, that the weather* has been uncommonly cold in Germany, England, and France, towards the end of last year, and the beginning of the present year, I have here sent you the degrees of altitude by Reaumur's thermometer, as I have observed them since the beginning of November 1767. The altitudes here set down are the arithmetical means of three taken every day, viz. in the morning before sun-rise, a little after noon, and at ten o'clock at night. Hence it appears that the cold has been moderate here with respect to this climate, and nothing more than common, though it was without intermission, from the beginning of December. The greatest cold in these months happened on the second day of January in the morning; the quick-silver then stood at 17,2 below the point of freezing.

* The least height of Fahrenheit's thermometer, set down in the course of the astronomical observations made at the Royal Observatory, was 15° on Jan. 6, at the transit of Venus over the meridian at 8 h. 42. m. A. M. At which time nearly the same was observed by my Lord Charles Cavendish in Great Malbro' Street, at 17°. But by a thermometer described in Vol. L. of these Transactions, placed on the top of the same house, in a very bleak situation, Dec. 31. it appeared to have been at 12½° in the preceding night; Jan 3. 16°; Jan. 6. 16°, and Jan. 7.

Day of the M.	Nov.	Dec.	Jan.	Feb.
1	6,3 +	4,2 —	13,3 —	2,3 —
2	5,2 +	6,4 —	14,1 —	6,1 —
3	4,5 +	4,3 —	8,2 —	4,1 —
4	2,4 +	4,4 —	4,3 —	3,5 —
5	2,2 +	6,8 —	9,7 —	5,5 —
6	1,9 +	8,6 —	6,0 —	0,4 —
7	1,0 +	0,7 +	4,0 —	3,7 —
8	3,5 +	0,6 —	9,8 —	0,4 —
9	6,3 +	5,0 —	7,6 —	0,4 —
10	7,5 +	5,2 —	11,2 —	2,6 —
11	5,9 +	0,2 +	7,2 —	1,4 +
12	4,6 +	2,0 +	5,8 —	2,7 —
13	6,0 +	3,4 —	6,0 —	5,8 —
14	3,7 +	1,8 +	1,7 —	6,6 —
15	5,5 +	3,8 +	0,4 —	4,0 —
16	4,0 +	4,6 +	1,0 —	6,8 —
17	5,6 +	3,4 +	0,9 —	6,9 —
18	5,3 +	0,8 +	1,2 —	5,9 —
19	2,4 +	0,6 +	1,3 —	3,2 —
20	2,6 +	1,6 —	2,1 —	1,8 —
21	4,3 +	2,5 —	3,5 —	2,5 —
22	4,4 +	2,5 —	1,5 —	3,1 —
23	3,0 +	4,5 —	1,0 —	
24	0,2 —	1,3 —	0,3 +	
25	1,8 +	1,6 —	0,2 —	
26	1,7 +	2,2 —	0,4 +	
27	2,4 +	3,0 —	0,8 +	
28	1,0 +	0,4 —	1,1 +	
29	0,5 —	2,7 —	1,0 +	
30	0,2 —	7,0 —	1,5 —	
31		9,6 —	1,0 —	

XXIII. *Introduction to two Papers of Mr. John Smeaton, F. R. S. by the Reverend Nevil Maskelyne, B. D. F. R. S. Astronomer Royal.*

Read May 12, 1768. **T**HE two following papers I received from my ingenious, and much esteemed friend, Mr. John Smeaton, with his desire, that I should communicate them to the Royal Society, if I thought they contained any hints conducive to the improvement of astronomy. As the first paper points out the time of observing the menstrual parallaxes of the planets in those circumstances in which they will be greatest, and at the same time shews how to obviate the error, which would otherwise arise from the inaccuracy of their theories (which must necessarily be used in the calculation), by correcting them from other observations, made on purpose, before and after the first mentioned observations; and the second paper gives a new and accurate method of observing the places of the heavenly bodies out of the meridian, independent of refraction, I apprehend they will prove acceptable presents to astronomers.

I shall only add one other remark, that has been suggested to me from the perusal of Mr. Smeaton's first paper; that, as it is there proposed to find the dimensions of the orbit described by the revolution of the Earth about its common centre of gravity and the Moon's, by means of the menstrual parallax of Mars, near his opposition, or of Venus, near her conjunction with

with the Sun ; the same may also be determined with advantage from the sum of the menstrual parallaxes of these two planets, when they happen to be in the required positions at the same time, which will indeed happen but seldom ; or even from the sum of the menstrual parallaxes of Mars and the Sun, which may be observed together at every opposition of Mars to the Sun ; the sum of the menstrual parallaxes of Mars and Venus in these circumstances, according to the numbers used by Mr. Smeaton, will sometimes amount to $87''$, and the sum of those of Mars and the Sun to $52''$.

Nevil Maskelyne.

XXIV. *A Discourse concerning the Menstrual Parallax, arising from the mutual Gravitation of the Earth and Moon; it's Influence on the Observations of the Sun and Planets; with a Method of observing it: By J. Smeaton, F. R. S.*

Read May 12, 1768. **I**T is demonstrated by Sir Isaac Newton in the *Principia*, that it is not the Earth's center, but the *common center of gravity* of the Earth and Moon, that describes the ecliptic; and that the Earth and Moon revolve in similar ellipses, about their common center of gravity. The same great author has also investigated, from the different rise of the tides, when the Moon is in conjunction or opposition to the Sun, to those which happen when the Moon is in her quadratures; that the quantity of matter in the Earth is to that in the Moon, as 39.78 to 1; from whence, and the known distance of the Earth and Moon, it would follow, that the common center of gravity of the two bodies falls without the surface of the Earth, by one half of its semidiameter: that is, that the center of the Earth describes an epicycle round the common center of gravity once a month, whose diameter is three semidiameters of the Earth.

Dr. Gregory, in his astronomy, has laid hold of this circumstance, in order to prove the relative gravity of the Earth and Moon, by observation; which is the subject of his 60th proposition of the fourth book;

book; in which he has demonstrated, that if an observer on the Earth makes a correct observation on the Sun's place, when the Moon is in one quadrature, it will differ from a like observation, taken in the opposite quadrature (according to a mean elliptic motion) by an angle which the diameter of this epicycle will subtend at the Sun. The same learned author has also shewn, in the scholium to the same proposition, that this quantity, or parallax, will be twice greater to Mars in opposition, and three times greater to Venus, in her inferior conjunction with the Sun.

The difference thus produced in the apparent place of the Sun, and of all the primary planets being governed by the Moon, and having it's period the same, may perhaps be not unaptly called the *menstrual parallax*.

Now if, with Sir Isaac Newton, the relative gravities of the Earth and Moon are taken between the proportion of 39 and 40 to one; the menstrual parallax of the sun will come out $13''$ upon the radius of the Earth's epicycle, and will affect the solar observations at the opposite quadratures, by double that quantity, viz. $26''$: in like manner, the mean distance from the Earth of Mars in opposition, being to the Sun's mean distance, as 1 to 2. 1; and the least distance of Mars from the Earth, to the Sun's mean distance, as 1 to $2\frac{1}{2}$, the menstrual parallax of Mars will affect the observation upon him in that situation, by $56''$ and $73''\frac{1}{2}$, respectively.

The mean distance of Venus from the Earth, in her inferior conjunction, being to that of the Sun as $3\frac{1}{2}$ to 1 nearly, and not very variable, on account
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of the orbit of Venus being almost circular; the menstrual parallax would affect the place of Venus, in that situation, by a quantity not less than $92''$; and in all other situations in proportion to her distance; which also holds with respect to all the rest of the planets.

These disturbing quantities are by no means to be dispensed with, in the nice and critical state that astronomical observations and calculations have arrived at, in consequence of the discoveries of Dr. Bradley, who may be said to have given a basis to astronomy; however, could we rely upon the *data*, on which Sir Isaac's investigation of the relative gravity of the Earth and Moon is founded, we should have nothing to do but to apply an equation to the particular cases, according to the diameter of the epicycle, as deduced from the relative gravity; but whoever considers the great obstructions that the water of the sea meets with in its motion to obey the influence of the Moon; the great difficulty in ascertaining the true height of the tides, from the many disturbing causes intervening; and the many uncertainties, and want of coincidence, that have attended, and must attend, such observations; must confess, that this matter does not seem capable of such a determination from that quarter, as the present state of astronomy requires.

Accordingly, since the time of Dr. Gregory, those great astronomers Dr. Bradley, De la Caille, and others, have applied themselves to determine the quantity of the menstrual parallax from solar observations: but though these have given cause to suppose that the relative gravity of the Earth and Moon are not above $\frac{2}{3}$ of the quantity deduced from the tides; yet, as the observation

observation of these small angles principally depends upon the observation of the Sun's right ascension (which, depending on the measure of time, is less capable of exact observation, than if depending on divided instruments); the deductions thence drawn seem still wanting of that certainty which the subject demands; and if to this we add, from a deduction of Mr. Maskelyne, that the relative gravity of the Earth and Moon is as 76 to 1, derived from the effect that the Moon produces in the nutation of the Earth's axis; the relative gravity, and consequently the parallaxes thereon depending, will be reduced to almost one half of those resulting from Sir Isaac's determination.

It is true, that the quantity of effect of the menstrual parallaxes will not be great, if computed upon Mr. Maskelyne's induction, for as much as that the common center of gravity will be considerably within the Earth's surface; yet, even in that case, the Sun's transit over the meridian, when the Moon is in one quadrature, will differ nearly one second of time from that observed in the opposite quadrature; and though De la Caille and Mayer have formed equations depending on the Moon, to be applied to the equation of time; yet, if we are at an uncertainty, whether the *maximum* of this equation is one second, two thirds of a second, or half a second of time, each way, we are still under a material difficulty; for though these differences are so small, that it is not easy to determine them exactly from solar observations; yet, as they are capable of creating a sensible difference in these observations, they will, so long as they remain undetermined, prevent that solidity and firmness

ness to the solar observations, which is the more necessary as they are the foundation of all the rest: but with respect to those planets, that in their periods come nearer to us than we to the Sun, the observations upon them will be affected by a greater uncertainty.

The determination of the menstrual parallax is of still more importance, as it is a necessary consideration in the determination of the Sun's parallax; and this, whether deduced from Mars or Venus, as I shall presently shew more particularly; but first I must state the quantity of the menstrual parallax, according to the best *data* yet known, by a contrary process; and, taking the mean quantity of the Sun's parallax, according to the determination of Mr. Short, at $8'' 8$, and the relative gravities of the Earth and Moon, according to Mr. Maskelyne, as 76 to 1, and the mean distance of their centers equal to $60 \frac{1}{2}$ semidiameters; we shall then have the distance of the Earth's center from the center of gravity, at $\frac{8}{10}$ of the Earth's semidiameter (that is, $\frac{1}{5}$ of that semidiameter within the Earth's surface) and the menstrual parallax equal to $\frac{8}{10}$ of the Sun's parallax; consequently about $7''$; and the double menstrual parallax, or vacillation, arising from the whole diameter of the epicycle, $14''$; the mean menstrual parallax of Mars in opposition, $29'' \frac{1}{2}$; the greatest, $38'' \frac{1}{2}$; and that of Venus $49''$; from hence it follows, that, was a person to attempt the Sun's parallax, by the diurnal motion of the Earth, applied as a basis to Mars in opposition, as has formerly been tried; and should the Moon be at new or full at the same time, the change of place of the Earth's center, in its own epicycle, would amount to an angle seen from Mars of

of $1''.3$ nearly ; that is, in case the interval between the observations was eight hours, and Mars at his mean distance ; but if Mars was not at his nearest distance, this change would in the same time amount to $1''.7$ nearly. In like manner, if a transit of Venus happens near the new or full Moon (as will be the case next year), the time of the transit will be affected by a change of place, such as the Earth's center will describe in its epicycle, during the time of the whole transit, if the beginning and end are observed in the same place ; or during the difference of absolute time, at which the transit appears to begin or end to different observers in distant meridians. Thus, when the same observer sees the beginning and end in the same place, the base described by that observer, from the Earth's diurnal motion, must be corrected by the space described by the Earth's center, in the circumference of it's epicycle, during that time ; which, if it be supposed of seven hours, will amount to an angle of $1''.9$, seen from Venus : but, where the beginning or end is seen by different observers in distant meridians, as the difference of absolute time can hardly amount to above 15 minutes, the change of place of the Earth's center will for that time be but small ; however, at the rate beforementioned, it will for 15 minutes affect the parallax angle seen from Venus, by about $\frac{7}{1000}$, of a second ; and the parallax of the Sun, by about $\frac{1}{4000}$ part of the whole : but this proportional part will remain the same, whether the distance of meridians be such as produce a greater or less difference of absolute time than 15 minutes*.

* If an error of $\frac{1}{1000}$ part of the whole may be supposed in the observation for determining the Sun's parallax by the transit

From what has been said, I suppose it will appear, that the effects of the menstrual parallax are worthy of consideration; and that nothing has been yet executed, whereby it has received a determination sufficiently accurate; for, in regard to observations upon the Sun, the whole quantity is too small to be minutely observed in right ascension: and with respect to the application to Mars and Venus, as suggested by Dr. Gregory, I do not know that any thing has been done; and indeed no wonder, as the theory of the motion of Mars and Venus has not been as yet so critically reduced to computation, as to render their parallaxes (though in themselves much greater) deducible with equal certainty as that of the Sun.

What I therefore have now to propose, is a method of observing the menstrual parallaxes of Mars and Venus, without laying any undue stress upon the theory of their motions.

The first opportunity of making an observation for this purpose, will be at the next opposition of Mars; which, according to the Nautical Almanack, will happen the 26th of October next, in the morning; I will therefore endeavour to illustrate this matter by taking that as an example.

The distance of Mars from the Earth will then be somewhat less than the mean distance, that is, as 1 to 2.2; and consequently his double menstrual parallax, according to Mr. Maskelyne, will be near $31''$ in the point of opposition. Now, as the Moon of Venus, a neglect of the menstrual parallax may make a $\frac{1}{10}$ part of the whole.

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will be at full, not above 12 hours preceding that opposition, the Moon will be nearly in the most favourable situation for the purpose.

For this end, let an accurate observation be made upon the place of Mars at the following times, viz. first, near the time of the new Moon, preceding Mars's opposition; or more properly at the nearest opportunity, to the time of the Moon's opposition to Mars; which will happen in the night, between the 12th and 13th of October: secondly, let the place of Mars be observed when the Moon is nearest her quartile with Mars; that is, between the 19th and 20th of the same month: thirdly, let an observation on Mars be made when the Moon is in conjunction with Mars, the nearest to his opposition with the Sun; that is, between the 25th and 26th of ditto: fourthly, let Mars again be observed when the Moon has moved on to her quartile with Mars, viz. between the 31st of October, and 1st of November: and fifthly and lastly, let the place of Mars be observed, when the Moon has again got to her opposition with Mars, which happens between the 7th and 8th of November.

Now it is manifest, that, when the Moon is in conjunction or opposition to Mars, the center of the Earth, the center of Mars, and the common center of gravity of the Earth and Mars, will be nearly in a right line, and consequently, that an observer will then see Mars, in the same place in the heavens, as if the common center of gravity was the same as the center of the Earth; therefore, then the place of Mars will be unaffected by a mensural parallax; and such will be the first, third, and fifth of the observations above propounded.

It is equally evident, that when the Moon is in quartile with Mars, and moving towards a conjunction, an observer, at the Earth's center, will see Mars more backward in the ecliptic, than if seen from the common center of gravity, by $15'' \frac{1}{2}$; and that, when the Moon is in her opposite quartile with Mars, and moving from her conjunction, that then an observer at the Earth's center, will see Mars advanced in his orbit more forward by $15'' \frac{1}{2}$, than if seen from the common center of gravity; and the one observation chequed with the other, will, according to a mean elliptic motion, differ by the quantity of $31''$; and such will be the second and fourth observations above propounded.

Now, from the first, third, and fifth, observations, three points of Mars's orbit will be given; which, by the help of the theory of Mars's motion in an elliptic orbit, whose aphelion, eccentricity, and nodes, are known sufficiently near for this purpose; the intermediate places of Mars may be inferred with the requisite degree of accuracy: and particularly, as the two intermediate observations, viz. the second and fourth, will be nearly at equal intervals of time between the three others: from hence it follows, that the difference between the inferred, or computed places, at the quartiles, and the observed places at those times, will be the menstrual parallax required.

It is to be noted, that the times above specified are the most favourable for the observation; and could those be made uninterruptedly from weather, there would be the less occasion for any other: but, as much as possible to prevent disappointments of this kind, it will be right to begin the observations, a month preceding

ing making the proper observations, at the conjunctions, quartiles, and oppositions, of the Moon with Mars, which will be the means of supplying such observations, as may happen to prove abortive before the opposition of Mars, and also, in case any of the observations to be made after that opposition shall prove deficient, the observations may be carried on for a month or competent time afterwards. As a further security against disappointments, as well as cheque, it will also be advisable to make the proper observations, the night preceeding and subsequent to those in which the quartiles, conjunctions, &c. happen; for, as the quantities will not differ considerably from those obtained on the days specified, with proper allowances they may be brought in support and confirmation of the former.

In like manner, when Venus is moving towards her inferior conjunction with the Sun, as will happen next year, the same observations may be made with respect to her; and continued for a necessary time, to get observations of the place of Venus; viz. the first, when the Moon is in conjunction or opposition with Venus: a second, when the moon is in her quartile with Venus: a third, in conjunction or opposition: a fourth, when the moon is in her opposite quartile to the former: and a fifth, again in conjunction or opposition: the same opportunity will also offer when Venus is moving from her inferior conjunction with the Sun, and becomes a morning star.

In regard to the observation of Venus, it is remarked by astronomers, that she is to be seen with a good transit telescope, when she is within a few degrees of the Sun; but, as she is three times nearer the Earth, than

than the Sun's mean distance, when her elongation is 25° in the inferior part of her orbit, it is plain, that the necessary observations may easily be made, when her mensrual parallax will be at a medium, three times greater than the Sun's; and consequently amounting for the whole difference to $42''$.

To avoid embarrassment in description, I have hitherto supposed, that all the observations are made in the meridian; in which case the right ascensions will be the same as they would appear from the center of the Earth; and consequently, the planet's longitudes thence deduced, nearly the same: but 'tis easy to see, that if the quartile observations are made when the planets are considerably to the east or west of the meridian, and so chosen, that the place of the observer be further distant from the common center of gravity, than the center of the Earth is from that center, that the base of the observations will be considerably enlarged. Thus, in our latitude, supposing that the quartile observations are made four hours before and four hours after the planet passes the meridian, this will produce an enlargement of the basis by one of the Earth's semidiameters: and as the whole base or diameter of the epicycle comes out, according to Mr. Maskelyne, no more than 1.6 of the Earth's semidiameters; the base will, according to this method, come out 2.6; and consequently, at the next opposition, the mensrual parallax of Mars will be thereby enlarged to $50''$, the greatest to $62'' \frac{1}{2}$, and that of Venus at a mean, to $74'' \frac{1}{2}$.

It must however be acknowledged, that no kind of observations of the places of the planets are of equal validity with those taken with the best instruments in
the

the meridian ; those taken with micrometers perhaps not excepted : for however accurately small distances can be measured by the micrometer of Mr. Dollond, yet, as these measures can hardly be reduced to the ecliptic, without having the difference of declination or right ascension from other means (except two stars making somewhat near a right angle with the planet should appear within the field of view at once) ; and as in all these cases the rectification of the places of the stars themselves ultimately depends on meridian observations we may perhaps be allowed to say, that in the most favourable cases of the micrometer, the determinations thence to be drawn, are not superior to meridian observations, and in less favourable cases, must be inferior : however, as the micrometer observations out of the meridian give an opportunity of repetition as often as we please ; and the observations for rectification of the stars concerned, can be repeated in the meridian, as often as we please also ; it must be equally allowed, that when these kind of observations are taken, not too near the horizon, when proper stars offer for this purpose, and the whole skillfully managed ; these kind of observations fall but little short of those taken immediately in the meridian. I cannot therefore hesitate to recommend, that the quartile observations be taken out of the meridian, as well as in it : in the first place, by Dollond's micrometer, if stars offer in proper positions ; and if not, secondly, by taking differences of right ascension and declination between the planet and the stars, by the common micrometer, in case proper stars offer themselves for this purpose : but as it frequently happens, that no proper stars offer themselves in micrometers

of

of either kind ; and this is still more likely to happen in the observations of Venus, which will be chiefly in the day light ; I beg leave to offer (what to me is) a new method of observation out of the meridian ; and which, though I esteem it not equal to micrometer observations of either kind, I apprehend will fall so little short thereof, and prove so much superior to any other method now in practice in these cases, that I hope I shall on this occasion be excused, in giving a particular description thereof : but, as it is a general method of observing out of the meridian, I shall reserve it by way of appendix.

In the next observation of Mars, it has been stated, that, in the meridian observations alone, the menstrual parallax, according to the smallest estimation, may be expected to amount to $31''$ in longitude ; which, turned into right ascension, will make about $2''$ of time : now, if it may be allowed, that a well-practised observer can take the time of a transit to $\frac{1}{4}$ part of a second, over a single wire, if he has three wires, or more, as usual, the mean of the three should be within $\frac{1}{4}$ part of a second ; or within $\frac{1}{16}$ part of the whole quantity in question : it is however a matter of chance, whether the mean of three may or may not be within $\frac{1}{4}$ part of the whole ; and as equal errors may be committed in the observations of the transits of the stars, wherewith the right ascensions of the planets in question are compared ; which it is an equal chance, whether they tend to correct or increase the errors committed in the former ; yet if, as has already been proposed, the observations are continued for two or three months, instead of one ; and observations taken the day preceding and subsequent to

to the days of conjunction, quartile, and opposition; and this as well out of the meridian as in it; we can hardly doubt but that, if the weather should favour, so many cheques would be formed, that, from the next opposition of Mars alone, the affair may be brought within a 24th part of the whole; and, if to this be added the force of such determinations, as may be drawn from observations on Venus, before and after her transit over the Sun next year, it can hardly be doubted, but that those three will bring us within a single second of a degree, subtended from the nearest planet; and these conclusions will be further strengthened by future observations; as two years will scarcely pass without affording one or more opportunities of this kind.

As I meant not to embarrass myself with exact computations, I have constantly supposed the distance of the common center of gravity from the center of the Earth, to be a fixed quantity; whereas it will vary in the same proportion as the Moon's distance varies; but, as this and many other *minutiæ* will properly enter the computation, when the observations are made, I must beg leave to refer them to the learned in this science.

Aulthorpe, April 17,
1768.

J. Smeaton.

XXV. *Description of a new Method of observing the heavenly Bodies out of the Meridian: By J. Smeaton, F. R. S.*

Read May 16, 1768. **T**HE instrument I propose for this purpose, is a transit telescope, mounted on a vertical axis; for example, such a one as is described in the introduction to the *Histoire Celeste* of Mr. le Monnier; being one of the instruments made by Mr. Graham for the academicians who went to measure a degree at the Polar circle; this or any other instrument upon equivalent principles will suffice, that is, capable of such adjustments, as to be made correctly to describe an almicanther and azimuth circle; and capable of being retained in any given position: the use will appear by the following example.

Make choice of any fixed star, which according to the diurnal motion, precedes the heavenly body to be observed by a few minutes, more or less, as it may happen; let the instrument be set to an azimuth, somewhat preceding the fixed star; and carefully observe the time of the star's transit cross the vertical wire of the telescope; then wait till the heavenly body comes to the same azimuth; and, when arrived within the field of view, keep gently turning the screw that alters the elevation of the telescope, so as to follow the heavenly body in altitude; keeping it intersected

by the horizontal wire of the telescope, till the body passes the middle vertical wire, and carefully note the time of its passage; there leave the telescope fixed as to altitude, and releasing the horizontal motion, turn it round on it's vertical axis, till you meet with some star, that in a little time after will by rising or falling come to the same almicanther; and, on it's arrival, carefully note the time of it's passage cross the horizontal hair of the telescope.

Now, from the right ascensions and declinations of the two stars being previously known, or afterwards determined from meridian observations; the azimuth of the first star, and the altitude of the last, at the time of their respective passages, may be determined by computation; which will give the altitude and azimuth of the heavenly body, for the time of the middle observation, when it passed the intersection of the two wires.

The same end may also be obtained by taking the observations in an inverted order; that is, by chusing a star at such an altitude, that the heavenly body shall in a competent time afterwards arrive at the same altitude, &c. but, as in these latitudes the alteration of azimuth is, especially in those parts that are in the neighbourhood of the zodiack, quicker than that of altitude, I apprehend it to be easier to follow the slower motion with the screw, so as to preserve the intersection, than the quicker, and therefore in general to be preferred; but where it happens otherwise, or the stars lay more conveniently, the inverse method may be pursued.

It is true, that some degree of dexterity and practice may be requisite in the observer in managing

the set screw, so as to keep the object intersected by the wire; but if fine smooth screws, such as are used for micrometers to astronomical quadrants, are adapted to the instrument, as well that commanding the horizontal motion as the vertical, I apprehend, the management will be perfectly easy and familiar to an observer otherwise well practised.

It is easy to see, that those stars are to be preferred that are nearest the heavenly body to be observed; and that, *cæteris paribus*, those in such positions, as rise or fall slow, are best for determining their altitude; and those that alter their azimuth slow, are best for determining the azimuth.

To avoid intricacy in description, I have supposed only two wires intersecting each other at a right angle, in the focus of the telescope: but, for the sake of getting a medium in such parts of the observations as depend on time, it will be proper to have, not only three perpendicular wires, parallel to each other as common, but also three horizontal wires; the proportional distances of which being previously determined by observation, the oblique motions may (in parts not near the pole) be considered as right lines.

This method is the more valuable as it is entirely free from the knowledge of refractions; for since the computation gives the real altitude from the time given independent of refractions; and since the heavenly body is equally affected by refraction, at the same altitude, the computed altitude of the star will give the real altitude of the heavenly body cleared of refraction, which never enters the question: and since such stars may be chosen as will render the time intercepted

tercepted short, there is the less chance of a change of refraction, during the time, between the middle and last observation; and therefore this method will be particularly useful in observations near the horizon:

Aufthorpe, April 17,
1768.

J. Smeaton.

N. B. Observations of this kind may be made upon the planets in the day light, by making use of the Sun for the first observation, instead of a star; and waiting afterwards for the appearance of the stars.

XXVI. *A Specimen of a new Method of comparing Curvilinear Areas; by which many such Areas may be compared as have not yet appeared to be comparable by any other Method.* By John Landen, F. R. S.

Read June 9, 1768. **W**HEN a body in motion is continually acted upon by a variable force, the space it has passed over at the end of any given time, it is well known, will be expressed by the area of the curve, whose ordinate expresses the velocity of the body, whilst the time it has been in motion is expressed by the corresponding abscissa. Therefore the facilitating the computation of curvilinear areas will manifestly contribute to the improvement of the doctrine of motion. Which doctrine being a branch of philosophy of no small importance, such improvement will not, I am persuaded, be looked upon as a trifling speculation, by the Royal Society: to whom, therefore, I do myself the honour of communicating this specimen of a new and ready method of computing such areas, by means of a given area; presuming that what is here written will not be deemed undeserving of a place in the Philosophical Transactions.

I.

Geometricians have found, that, if A be put to denote the whole area of the curve, whose abscissa is x , and ordinate $1 - x^p - 1 \times x^{2p} - 1$, the

the *whole* area of the curve, whose abscissa is x , and ordinate $\sqrt[p]{1-x^{2p-1}} \times x^{2n+2r-1}$ will be $= \frac{r.r+1.r+2(n)}{p+r.p+r+1.p+r+2(n)} \times A$; n being any positive integer, and p and r any positive numbers, whole or fractional.

II.

By the preceding article, the *whole* area, when the ordinate is $\sqrt[p]{1-x^{2p-1}} \times x^{2r+2z-1}$ is $= \frac{z.z+1(r)}{p+z.p+z+1(r)} \times \frac{1.2(z-1)}{p.p+1(z)} \times \frac{1}{2}$; the *whole* area, when the ordinate is $\sqrt[p]{1-x^{2p-1}} \times x^{2z-1}$, being $= \frac{1.2(z-1)}{p.p+1(z)} \times \frac{1}{2}$.

Likewise, by the same article, the same *whole* area is $= \frac{r.r+1.r+2(z)}{p+r.p+r+1.p+r+2(z)} \times A$. Therefore this last expression is $= \frac{z.z+1(r)}{p+z.p+z+1(r)} \times \frac{1.2(z-1)}{p.p+1(z)} \times \frac{1}{2}$. From which equation, p and r being positive, as before observed, A , the *whole* area of the curve, whose ordinate is $\sqrt[p]{1-x^{2p-1}} \times x^{2r-1}$, is found equal to $\frac{1.2.3(r+z-1) \times p+r.p+r+1(z)}{p.p+1.p+2(r+z) \times r.r+1(z)} \times \frac{1}{2}$, being any number whatever.

Consequently, supposing z infinite, we find $A =$ the ultimate value, or limit of $\frac{1.2.3(z) \times p+r.p+r+1(z)}{p.p+1.p+2(z) \times r.r+1(z)} \times \frac{1}{2z}$.

Having thus obtained a general expression for the *whole* area of any curve, whose ordinate is expressed by $\sqrt[p]{1-x^{2p-1}} \times x^{2r-1}$, and that expression

expression for such area consisting of an infinite number of factors multiplied together; to render the same useful in practice, some theorems are requisite for ascertaining the limits of such products. The theorems which I have hitherto been able to investigate suitable to that purpose, I shall give in the next two articles.

III.

The limit of $\sqrt{1-m^2} \times \sqrt{2^2-m^2} \times \sqrt{3^2-m^2} (z) \times \frac{N^{2z}}{2^z+1}$ is $= \frac{2}{m} \times$ sine of mS ; N being the number whose hyp. log. is 1, and S the semi-periphery of the circle, whose radius is 1.

Whence, by taking m equal 0, we find the limit of $1^2 \cdot 2^2 \cdot 3^2 (z) \times \frac{N^{2z}}{2^z+1} = 2S,$

IV.

The limit of $\sqrt{dz+a} \times \sqrt{dz+a} \times \sqrt{dz+a} \times \sqrt{dz+a} \times \sqrt{dz+a} \times \frac{N^{2z}}{2^z d^z z^z}$ is $= 2^{\frac{a}{d} - \frac{1}{2}}.$

Hence, $\sqrt{z+1} \cdot \sqrt{z+2} \cdot \sqrt{z+3} (z)$ being $= 1.3.5 (z) \times 2^z$, it appears, that the limit of $1.3.5 (z) \times \frac{N^{2z}}{2^z z^z}$ is $= 2^{\frac{1}{2}}.$

I shall now give some examples, to shew the use of the above

Writing

V.

Writing A, B, C, D, and E, for the *whole* areas of the curves, whose ordinates are $\frac{x^3}{1-x^2}^{\frac{1}{2}}$, $\frac{x^3}{1-x^2}^{\frac{1}{2}}$, $\frac{1}{1-x^2}^{\frac{1}{2}}$, $\frac{x^3}{1-x^2}^{\frac{1}{2}}$, and $\frac{x^3}{1-x^2}^{\frac{1}{2}}$, respectively; we have, by Art. II.

$$A = \text{the limit of } \frac{1.2.3(z) \times 4.7.10(z)}{1.3.5(z) \times 5.11.17(z)} \times \frac{2^{2z-1}}{z};$$

$$B = \text{the limit of } \frac{1.2.3(z) \times 7.13.19(z)}{1.3.5(z) \times 2.5.8(z)} \times \frac{1}{2z};$$

$$C = \text{the limit of } \frac{1^2.2^2.3^2(z)}{1^2.3^2.5^2(z)} \times \frac{2^{2z-1}}{z} = \begin{cases} \text{the area of the femi-circle,} \\ \text{whose radius is 1;} \end{cases}$$

$$D = \text{the limit of } \frac{1.2.3(z) \times 5.11.17(z)}{1.3.5(z) \times 1.4.7(z)} \times \frac{1}{2z};$$

$$E = \text{the limit of } \frac{1.2.3(z) \times 2.5.8(z)}{1.3.5(z) \times 1.7.13(z)} \times \frac{2^{2z-1}}{z}.$$

Now it appears by the above equations, that $\frac{A}{B}$ is = the limit of $\frac{2.4.5.7.8.10(2z)}{5.7.11.13.17.19(2z)} \times 2^{2z}$; which by Art. III. is = $\frac{6 \times \text{fine } 60^\circ}{12 \times \text{fine } 30^\circ} = 3^{\frac{1}{2}}$.

$$\text{Therefore A is } = \frac{3^{\frac{1}{2}} B}{2}.$$

It appears also, that $\frac{B \times D}{C}$ is = the limit of $\frac{5 \cdot 7 \cdot 11 \cdot 13 \cdot 17 \cdot 19}{2 \cdot 4 \cdot 5 \cdot 7 \cdot 8 \cdot 10} \frac{(2z)}{(2z)} \times \frac{3}{2^2 z + 1}$;

which by Art. III. is $= 3^{\frac{1}{2}}$.

Therefore D is $= 3^{\frac{1}{2}} \frac{C}{B}$.

It likewise appears, that

$B \times E$ is = the limit of $\frac{1^2 \cdot 2^2 \cdot 3^2 (z)}{1^2 \cdot 3^2 \cdot 5^2 (z)} \times \frac{3 \cdot 2^{2z} - 1}{z} = 3 C$.

Therefore $E = \frac{3 C}{B}$.

VI.

Writing F, G, H, I, and K, for the *whole* areas of the curves, whose ordinates are $\frac{x^{\frac{2}{3}}}{1-x^{\frac{1}{3}}}$, $\frac{x^{\frac{1}{3}}}{1-x^{\frac{1}{3}}}$, $\frac{1}{1-x^2}$, $\frac{x^{-\frac{1}{3}}}{1-x^{\frac{1}{3}}}$, and $\frac{x^{-\frac{2}{3}}}{1-x^{\frac{1}{3}}}$, respectively, we have, by Art. II.

$F =$ the limit of $\frac{1 \cdot 2 \cdot 3 (z) \times 9 \cdot 15 \cdot 21 (z)}{2 \cdot 5 \cdot 8 (z) \times 5 \cdot 11 \cdot 17 (z)} \times \frac{3^z}{2^z}$;

$G =$ the limit of $\frac{1 \cdot 2 \cdot 3 (z) \times 8 \cdot 14 \cdot 20 (z)}{2^2 \cdot 5^2 \cdot 8^2 (z)} \times \frac{3^z}{2^z + 1}$;

$H =$ the limit of $\frac{1 \cdot 2 \cdot 3 (z) \times 7 \cdot 13 \cdot 19 (z)}{2 \cdot 5 \cdot 8 (z) \times 1 \cdot 3 \cdot 5 (z)} \times \frac{1}{2^z} = B$;

I = the

$$I = \text{the limit of } \frac{1^2 \cdot 2^2 \cdot 3^2 (z)}{2 \cdot 5 \cdot 8 (z) \times 1 \cdot 4 \cdot 7 (z)} \times \frac{3^{2z}}{2^z},$$

$$K = \text{the limit of } \frac{1 \cdot 2 \cdot 3 (z) \times 5 \cdot 11 \cdot 17 (z)}{2 \cdot 5 \cdot 8 (z) \times 1 \cdot 7 \cdot 13 (z)} \times \frac{3^z}{2^z}.$$

By which equations it appears, that $\frac{F}{H} = \frac{F}{B}$ is = the limit of $\frac{1 \cdot 3 \cdot 3 \cdot 5 \cdot 5 \cdot 7 (2z)}{5 \cdot 7 \cdot 11 \cdot 13 \cdot 17 \cdot 19 (2z)} \times 3^{2z}$; which by Art. III. is = $\frac{4 \times \text{fine of } 90^\circ}{12 \times \text{line of } 30^\circ} \frac{2}{3}$.

$$\text{Therefore } F \text{ is } = \frac{2B}{3}.$$

It appears likewise, that

$$\begin{aligned} \frac{G}{B} & \text{ is } = \text{the limit of } \frac{1 \cdot 3 \cdot 5 (z) \times 8 \cdot 14 \cdot 20 (z)}{2 \cdot 5 \cdot 8 (z) \times 7 \cdot 13 \cdot 19 (z)} \times \frac{3^z}{2^z} \\ & = \text{the limit of } \frac{1 \cdot 3 \cdot 5 (z) \times 4 \cdot 7 \cdot 10 (z)}{4 \cdot 7 \cdot 10 (2z)} \times 6^z \\ & = \text{the limit of } \frac{1 \cdot 3 \cdot 5 (z)}{3^z + 4 \cdot 3^z + 7 \cdot 3^z + 10 (z)} \times 6^z; \end{aligned}$$

which, by Art. IV. is = $2^{-\frac{1}{3}}$.

$$\text{Therefore } G \text{ is } = \frac{B}{\frac{1}{2}}.$$

It also appears, that

$$\begin{aligned} I & \text{ is } = \text{the limit of } \frac{1^2 \cdot 2^2 \cdot 3^2 (z)}{2 \cdot 3 \cdot 5 \cdot 7 \cdot 8 \cdot 10 (2z)} \times \frac{3^{2z+1}}{2}; \text{ which, by} \\ \text{Art. III.} & = \frac{2S}{4 \times \text{fine of } 60^\circ} = \frac{S}{3^{\frac{1}{2}}} = \frac{2C}{3^{\frac{1}{2}}}. \end{aligned}$$

A a 2

Moreover

Moreover it appears, that

$\frac{B \times K}{I}$ is = the limit of $\frac{1 \cdot 4 \cdot 7}{2 \cdot 5 \cdot 8} \frac{(z)}{(z)} \times \frac{5 \cdot 11 \cdot 17}{1 \cdot 3 \cdot 5} \frac{(z)}{(z)} \times 3^z$
 the limit of $\frac{3z+2 \cdot 3z+5 \cdot 3z+8}{1 \cdot 3 \cdot 5} \frac{(z)}{(z)} \times \frac{1}{2^{z-1}}$; which,

by Art. IV. is $\frac{3}{2^{\frac{1}{3}}}$.

Therefore K is $\frac{3 I}{2^{\frac{1}{3}} B} = \frac{2^{\frac{2}{3}} \cdot 3^{\frac{1}{3}} C}{B}$

And, in like manner, may a great number of other areas be compared.

Note. All the *whole* areas above-mentioned are supposed to begin where x begins, and to stand upon a base = 1.

Received December 12, 1765.

*XXVII. *Experiments and Observations upon a blue Substance, found in a Peat-moss in Scotland: By Sylvester Douglas, Esquire.*

Read February 13, 1766. **T**HE blue coloured substance, which is the subject of the following observations, and which is now before you, was accidentally dug up in the summer 1759, in order to mix with some other materials for the purpose of manure, to be laid on some ground, at present in my possession, in the north of Scotland, about twelve miles from Aberdeen.

I have not met with a description of this substance in any naturalist. Kentman indeed in a few lines mentions a blue earth, which he calls *cæruleum Patavinum*, which agrees with the substance I am about to describe, in one remarkable circumstance; that it is at first of a white colour, and becomes blue only in consequence of being exposed to the air. Mr. Da Costa's *ochra friabilis cærulea* †, would also have been found probably to correspond with it, if a more particular account could have been given of the circumstances in which it was found, and its appearance before the air had acted upon it. Mr. Cronstedt, in

* This paper, having being mislaid, could not be printed in the Volume of the Philosophical Transactions for 1766.

† Nat. Hist. of Foss. p. 103.

his late System of mineralogy, mentions a blue substance, which seems to be of the same kind, and which, I think, he says, is found somewhere in Prussia. His account of it is very short; and I am not very certain with regard to it, as I have not the book by me.

The place, where it is dug up, is of a marshy nature, in the corner of an exhausted peat-moss. Immediately under the sward lies a *stratum*, about a foot deep, of common peat; next to that is the substance itself, with irregular *striae* of a peaty matter all through it, to the depth of near another foot; and below this, I think, there is clay. While it is thus wet, and shut out from the air, it is of a white colour, and seemingly of a fatty consistence, not unlike lime that has been prepared for cement. All the water in the neighbourhood of the place is in some measure impregnated with iron. When this substance is exposed to the air, it gradually as it dries assumes the blue colour; the peaty matter intermixed with it continuing of the same appearance as before. The whole mixed mass is of a very friable texture, easily crumbling betwixt the fingers; and the blue part, gently rubbed between them, feels like a fine impalpable powder. It has hardly any sensible taste; what it has, approaches a little to that of sulphur: the smell, when it is first taken up, is sensibly sulphureous, and if a piece of paper, with part of it adhering to it, be kindled, it shews a flame similar to burning sulphur.

The only means of separating it from the black matter is by elutriation. When water is poured on it, and they are shaken together, and then left at rest for some time, the black part subsides to the bottom,

and

and the blue can be poured off still diffused in the water, from which however it soon separates, and falls to the bottom. It is not possible entirely to free the blue from the peaty matter, for, after above twenty different additions of water, there were still streaks of black interspersed through it, when it was allowed to subside; neither have I ever been able to separate all the blue from any of the black part.

When a little water is added to a quantity of it, it acquires some degree of tenacity, and when a small portion of water is allowed to stand on its surface for a day or two, the water becomes of a yellowish colour.

These are the chief circumstances relating to its natural history, and the obvious properties it discovers without the assistance of chemical operations. The following are the experiments I have made upon it, with a view to discover its nature more particularly.

In order to find whether there was any part of it soluble in water, I passed a large quantity of water, which I had used in separating the black from it, through a filter, and then set it to evaporate in B M; but there was nothing left in the vessel after the evaporation, except some earth, which the water had probably contained in itself.

To a quantity of the blue powder, I added the common vitriolic acid of the shops; a degree of effervescence ensued, and a considerable froth remained for some time on the surface; the whole was changed into a dark brown colour, and, when filtrated, the solution was a transparent brown liquor. A considerable sediment remained behind on the filter; but I am inclined to think, that this consisted chiefly of the
peaty

peaty matter, which had not been entirely separated; for when the experiment was repeated several times with different parcels of the blue, it appeared more or less soluble according as the black had been more or less perfectly separated; and when I added the vitriolic acid to a quantity of the black, though it turned it all of a brown colour, it only seemed to dissolve a quantity equal to the portion of blue, which still adhered to it.

The nitrous acid, added to the blue powder, produced pretty much the same effects, only the filtrated solution was of a much lighter brown.

The fixed vegetable alkali dissolved also a considerable part of it; but whether the whole or not, I cannot say. The solution was an opaque brown liquor, which did not become transparent after being twice filtrated, though it deposited no sediment upon standing several days.

I added a small quantity of volatile alkali to it, which seemed to dissolve part of it, and turned the rest obscurely green.

To the solution in vitriolic acid, I joined some fixed vegetable alkali: an effervescence arose, and a light curd of a colour between green and blue was thrown to the top, which soon subsided, and became white.

A similar præcipitate was obtained from the nitrous acid, only it was not at first thrown up to the surface in the same manner as the foregoing.

From the solution in fixed vegetable alkali, a reddish brown præcipitate was obtained, by the addition of vitriolic acid. Equal quantities of the blue powder and of black flux were mixed together, and being

put into a crucible, were kept in a strong degree of heat for several hours: on being removed, and taken out of the crucible, the whole was found concentered into a spongy mass, the bottom of which was crufted over with something that had a kind of metallic appearance. This mass was powdered, and the lighter parts washed off; after which, a magnet was applied to what remained, and it attracted many of its particles strongly, without being brought in contact with them.

Part of the white præcipitate from vitriolic acid was mixed with a little fixed alkali, and being laid on a piece of charcoal, the flame of a candle was directed to it by means of a blow-pipe. It was thus kept in a red heat for about an hour; and on being removed, the magnet was applied to it, but none of the powder was attracted by it. The quantity that can be examined in this way does not exceed a few grains.

To a small quantity of the white præcipitate, I added an infusion of tea; which turned it blue, approaching to the original colour, but not so deep.

To another parcel of the same, I added some infusion of galls, and shook them together. The liquor became of a dark blue colour, and what part of the powder remained at the bottom of the glass was of the same colour. This was not so bright as that of the original powder diffused in water, but entirely such as might be expected from the diffusion of it in a brown liquor like infusion of galls; and, to shew this, I poured some of the infusion of that astringent on the blue substance itself, and on shaking them together, they produced a colour almost entirely the same.

A quantity of the brown solution in vitriolic acid was diluted with water till it became very pale. I then poured to it some infusion of galls, which turned it immediately black.

A parcel of the blue substance, being placed at the distance of a foot from the fire, was changed to a greenish colour.

These experiments, compared with its natural history, seem to throw some light on the nature and composition of this curious production. It is the known property of all vegetable astringents, to affect the colour of iron, either when it is combined with vitriolic acid, in the form of green vitriol, or by itself; and I believe they have no such effect on any other metal. The colour they produce with it is various, inclining indeed to black, but almost of every different shade between black and blue; and it seems to me, that they occasion a more pure black with vitriol, and a purple blue with iron itself, as is seen for instance on dropping a little infusion of tea on a knife.

Now we find, that when a vegetable astringent is added to a solution of this substance in vitriolic acid, it strikes a black colour with it, and restores the original blue to the white præcipitate from that acid. We also find, that there actually is iron contained in it; because, when fluxed with the black flux, its particles are attracted by the loadstone; and we can draw no argument, from our not having discovered iron in the experiment with the blow-pipe, against the presence of that metal, as so little can be examined in that way. I therefore think it probable, that the principal ingredients of it, and those on which its colour

colour depends, are iron and some vegetable astringent. The situation in which it is found favours this conjecture very strongly; for, in the first place, the water in the neighbourhood of it is all impregnated with iron; and secondly, in almost every peat-moss, there are the remains of oak trees, still fresh dispersed through them; and both their wood and bark are of a strong astringent nature.

I do not pretend to say, that these are its only ingredients. I think we may conclude, from the lightness of the substance, that iron does not form a very great part of it; and the smell, and the particular flame it exhibits in burning, would seem to shew the presence of sulphur in it. This, however, can be only in a very small proportion, since so much of it is soluble in acids, which do not at all affect sulphur. I suppose the præcipitate from acids consists chiefly of iron and earth.

I have made some trials on the blue powder after it was partly well freed from the black matter, in order to see how far it might be useful as a paint: a quantity of it was rubbed in a glass mortar with oil of walnuts; but, after being thoroughly mixed with the oil, its colour was changed to black. It is probable, therefore, that little can be expected from it as an oil colour; but it retains its natural brightness when mixed with gum-water; and, as it is naturally in a very fine powder, it is diffused intimately through it without any difficulty, so that, if it could be got in sufficient quantity, it would be a cheap and usefull water-colour. I think there is reason to believe, that it might be found in most peat-mosses, as what seem to be the materials of which it is composed are pre-

sent almost in all of them. Two or three years ago a gentleman sent me a parcel of it, which he found in a moss on his estate, five or six miles distant from the place where I first observed it. I am informed, that Mr. Da Costa has had specimens of a blue earth sent him from different parts of England: what Sir Hans Sloane gave him from Ireland seems also to have been the same, and, from what I have quoted from Kentman and Cronstedt, it would appear, that it is obtained on several parts of the continent. From all this, I think we may conclude, that it might be procured in sufficient quantity to be a cheap paint; particularly as it is in a manner levigated and prepared by nature.

It is to be lamented, that its colour is so easily affected by alkalies, especially the volatile alkali, which abounds so much in the atmosphere in towns, and by any considerable degree of heat. I have, never however, found any change produced on it from being exposed for a considerable time to the air, or to the heat of a room where a fire was kept constantly burning.

XXVIII. *Two Medical Observations by Dr. Joseph Benevuti, Physician at Lucca; communicated to the late President of the Royal Society, by Dr. Ch. Allioni of Turin, F. R. S. and translated from the Latin by Daniel Peter Layard, M. D. Physician to her Royal Highness the Princess Dowager of Wales, Member of the Royal College of Physicians in London, and of the Royal Societies of London and Gottingen.*

I. *Of a sick Man surprizingly recovered from a Fever.*

Read June 9, 1768. **A** MAN forty years of age, named Angelus Amadei, of a plethoric constitution, and of a low size, having a malignant fever, began on the ninth day to grow delirious, and continued so during the tenth night; when, several bad symptoms appearing, it was thought he must die soon. Early on the eleventh day in the morning, he bid the by-standers quit his room, and expressed a desire of going to sleep; his friends were unwilling to withdraw, unless they first stripped him of his shirt, and dried him of the sweat he was in. But the patient refusing, and at last growing angry, they were obliged to yield to his will. About an hour after, a woman went into the bed-room, and not finding

finding the man, she called the servants, who searched the house, and the well, into which they feared he had thrown himself; but to no purpose. In the mean time a rumour spread, as is usual in such cases, that this had happened, either by the interposition of the devil, or by a miracle.

The keeper of the baths at Lucca gave orders for every body to make a diligent search; and on the third day the sick man was at last found in a vineyard, about two miles from his house, hidden in a hut, where, he said, that the day before, he with great astonishment found himself, without at all knowing how he came there. It seemed to me, that he must have got down by the window of the bed-chamber, which was not far from the ground. What seems most extraordinary is, that, in order to quench his thirst, this man swallowed a large quantity of snow (with which the earth was covered, it being in the winter); and that neither this sort of drink, nor the cold air, did in the least affect him; for though he had gone away from home all in a sweat, and with no other covering than his shirt, yet he was freed from his fever, and is now restored to his former health.

II. *Of an extraordinary great Head.*

Not long since, I went to Benabii, a town situated in the territory of Lucca, to see a man, whose head, I had heard, was much larger than is usual. The same curiosity procured me the honour of attending at the same place on Princess Lambertini, niece to Pope Benedict the XIVth, whose health I had the care of, while she drank the water of our baths.

I saw

I saw a man, thirty years of age, and yet of the size of a boy seven years old, who was sitting on a couch seat, with his head (which indeed was quite out of size) inclined on the right side, and resting on a pillow; which, when he wanted to move, he supported with his hands, as it lay on a very small neck. This man had enjoyed a good health till he was six years old; he then had a diarrhoea, which lasted nine months, and, upon its stopping, his lower extremities were seized with the palsy, and lost their motion, but their feeling remained. From that time his head increased yearly, together with his face, nose, ears, eyes, mouth, &c. but the remainder of his body did not grow at all. The circumference of his scalp measured thirty seven inches, and eight lines, English measure. The length of his face was twelve inches and three lines. These measures were taken by the said princess and several of her attendants. This man eats greedily, sleeps well, but discharges his *faeces* and his urine involuntarily. The strength which he has in his hands is very surprizing, being such, that it is difficult for any person to get loose from him, when once he holds fast. He is besides quick as to his understanding, he talks, and has a good memory; seldom or never forgetting what he may have read in books.

XXIX. *An Account of a particular Species of
Cameleon: By James Parsons, M. D.
F. R. S.*

Read June 12,
1768.

AMONG the quadrupeds of the earth, the class of Cameleons is one of the most curious families; insomuch as to have engaged the attention of many natural historians; not only on account of the particular structure of its parts, but also of several curious phænomena which are peculiar to it, in its several species, in the different parts of the world.

This animal is ranged by authors under the generical name *Lacerta*, which comprehends a great variety of all sizes from the Crocodile to the smallest Lizard: but as the Cameleon has its various species, and each such properties as are not common to any others under the tribe of *Lacerta*, they indeed deserve to be regarded as a particular *genus*.

However, since authors have been very full in their accounts of these creatures; which every one, curious in their enquiries into the history of animals, may have recourse to, collected in an excellent work intitled, *Dictionnaire raisonné des Animaux*, I shall only entertain the learned Society with a description of a species of Cameleon which I consider as a non-descript, having made a careful research concerning this animal among authors, and seen several kinds of them, as well as various figures in every history I am acquainted with; from all which the subject before us is very different.

It

It is chiefly in the structure of the head that this difference appears, and its singularity induced me to observe it with attention; for the head is very large in proportion to the rest of this animal, and all others of the same class; and the more so, if we measure from the two anterior flat processes, to the posterior extremity or process of the *cranium*, which measures three inches and a quarter. . . This posterior process extends backwards, over the neck, to the first vertical process of the spine; and the interior processes, one on each side, project forwards and upwards in an oblique direction over the nasal hole, and are bluntly serrated all round; the surface of the entire face is covered with tubercles and scales, which, by being in a dry state, have lost their protuberance and lustre, which the scales certainly were endowed with while the animal was alive.

The length of the two mandibles is equal, and is two inches and a quarter from the articulation of the lower with the upper jaw, to the apex of each; both being furnished with a fine set of small pointed teeth, all of a size, and so set, that, upon the animal's closing his mouth, the teeth do not meet, but those of the upper fall in with those of the under alternately. There are no molares nor canine teeth.

The orbits are extremely large and deep, so that this Cameleon must have had very great eyes, and very globular; for they are each more than a third of the whole length of the mandible in diameter.

From a close inspection of the skin, which is now contracted and dried close to the skeleton, it appears scaled all over; the larger scales are upon part of the head and upon the sides of the neck; the smaller, under the jaws, upon the neck, and over the whole

body ; but we can form no idea of its proper colour whilst the animal is alive, yet do not doubt of its having had a very beautiful covering.

Almost every species of *Lacerta* have five fingers upon each extremity ; all the Cameleons have them, but they differ in the disposition of the fingers ; this before us has the tarsal, metatarsal, and three bones to each finger, as it is in human hands : in this Cameleon the fingers are very long, and terminated with pointed nails bending downwards ; three of the fingers of each anterior extremity are inwards in the place of the thumb, and the other two are outwards ; whereas in the posterior extremities, three are outwards, and the other two inwards, having between them such a large space, or division, as is between the thumb and fingers of men.

But this distribution of the fingers I saw in one of the triangular-headed Cameleons : other species have the five fingers together, and very short like stumps ; but that described by Pittfield from the dissections of the Royal academy, has its fingers disposed in the same manner with this, and is one of those with a triangular head and crest.

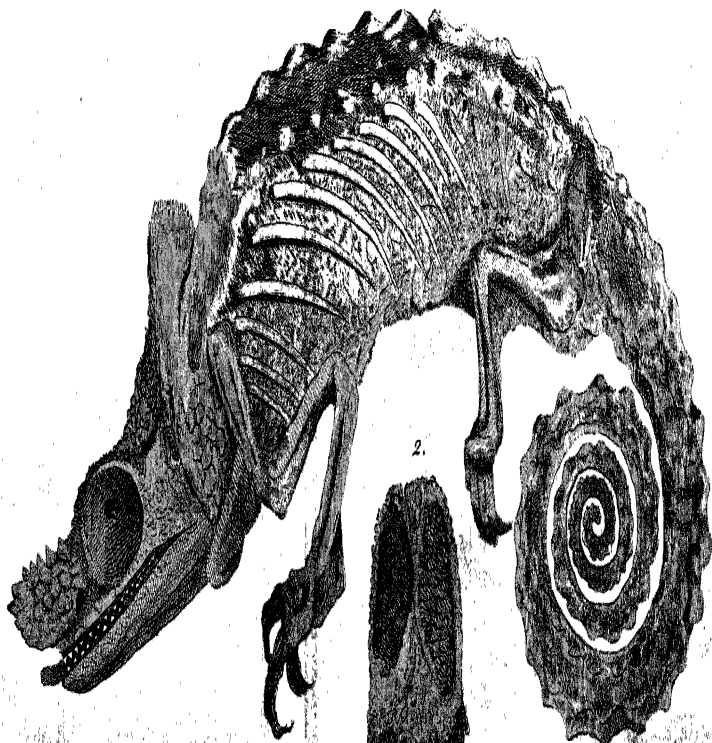
The vertical edge of the spine is scolloped all along from the neck to the extremity of the tail, and has on each side a row of knobs, or processes, as far as the articulation of the thigh with the bone that runs up towards the spine ; but from thence, where the tail begins, there is a second lateral row of knobs, which continue all along the tail.

There does not appear any passage into the head for hearing, nor any other but the mouth and nasal holes ; which is also taken notice of by the Royal Academy

Cameleonis variflora Species
a Jacobo Parsons M.D. Jam primo
descripta. 1768.

Philos. Trans. Vol. LVIII. TAB. VIII. p. 195.

1.



2.



Academy in their observations upon that mentioned above. This made Bellonius imagine, that these nasal holes serve Cameleons for hearing as well as breathing; so that it should seem, that more species than one are destitute of auditory holes.

This subject came into my hands from the owner Mr. Millan, who was kind enough to leave it with me for the purpose of laying it before the Royal Society; we have no knowledge of its native place, as he bought it among other natural productions now in his collection.

T A B. VIII.

The first figure represents the animal in profile.

The second is a view of the face, or upper surface of the head.

XXX. *A Letter from J. A. Rizzi Zannoni, Member of the Academy of Sciences at Gottingen, and Geographer to his Sicilian Majesty, to the late Earl of Morton, Pr. R. S. containing several Astronomical Observations, made in several Parts of the Kingdom of Naples and Sicily ; translated from the French, by Mathew Maty, M. D. Sec. R. S.*

Naples, July 29, 1768.

My Lord,

Read Nov. 10, 1768. **I** Take the liberty to apply to your Lordship on a subject of great importance to geography. His Sicilian Majesty has lately ordered a topographical map of his dominions to be made; and all the materials, which are to serve for that work, have been collected together.

We frankly acknowledge, my Lord, that we are indebted to Englishmen for most of the astronomical observations, made in various parts of the kingdom of Naples. The following list contains them all; but, in order to render them useful, it would be necessary to have the corresponding ones made at London, or Greenwich, the latitude and longitud of these two places being perfectly known, by the accurate observations of the mathematicians belonging to the Royal Society.

We,

We, therefore, beg of your Lordship to procure us, from the members of that illustrious body, the observations relative to our object. As for the computation of the parallaxes, and the results, I will set about that work myself, as soon as I shall be furnished with the proper materials.

I have the honor to be,

My Lord,

Your Lordship's

most obedient humble servant,

J. A. Rizzi Zannoni.

Eclipses of the three first Satellites of Jupiter, observed at the Royal College at Naples, in the years 1762, 1763, and 1764, by Father Maria Carcani, Superior of the Pious Schools, by means of a telescope of 24 palms *.

	August	4	at	1	35	9	
	Sept.	26	—	4	2	17	} Immerf. of I. Satellite.
	Octob.	19	—	4	19	44	
	Nov.	14	—	7	43	19	
					or	20	
1762	—	28	—	11	24	52	} Emerf. of I.
	—	30	—	5	52	14	
	—	30	—	10	45	49	
	Decem.	14	—	9	36	33	Emerf. of I.
	—	25	—	7	47	56	Emerf. of II.

	Jan.	6	—	9	39	36	
	—	22	—	7	54	7	} Emerf. of I.
	—	29	—	9	47	35	
	March	9	—	8	26	49	
1763	—	9	—	8	11	9	Emerf. of III.
	—	25	—	6	55	24	Emerf. of I.
	Octob.	12	—	15	54	25	Immerf. of II.
	—	14	—	17	55	45	Immerf. of I.
	—	30	—	10	27	31	Immerf. of II.

	Jan.	4	—	5	39	3	
	—	25	—	11	15	23	
	Feb.	10	—	9	33	0	
	—	17	—	11	29	0	
1764	—	19	—	5	58	47	} Emerf. of I.
	—	26	—	7	55	30	
	March	4	—	9	51	25	
	—	13	—	6	18	8	
	—	27	—	10	14	51	

* The Italian palm is about two-thirds of a foot.

1760	Aug.	11	at 10	17	20	Imm. of I.	} Emerf. of I. } at Messina, by Mr. Robert Dunn.
	—	20	—	8	57	28	
	—	27	—	10	52	47	
	Sept.	3	—	12	49	55	} Emer. of I. at Reggio, by Mr. Dunn.
	—	11	—	2	48	5	
	—	12	—	9	17	49	} Emer. of I. at Cotrone, by Mr. Dunn.
	Oct.	5	—	9	46	11	
	—	12	—	11	44	34	
	July	3	—	11	45	10	Imm. of I. at Malta, by Mr. Dunn.

1762	Sept.	11	—	12	16	38	} Imm. of I. at Policastro, by Mr. Berkeley.
	—	19	—	2	12	36	
	Oct.	4	—	12	35	53	} Imm. of I. at Cirella, by Mr. Berkeley, with a Newtonian Reflector of 3 feet, of Watkins.
	—	12	—	2	31	37	
	—	13	—	9	0	31	
	—	19	—	4	25	56	
	—	20	—	10	56	37	

1764	May	5	—	9	20	49	Em. of I. at Santa Maura.
	Aug.	10	—	15	14	6	Imm. of I. at Linguetta.
	Sept.	2	—	15	29	51	} Imm. of I. at Durazzo, by Mr. Berkeley.
	—	3	—	15	54	50	
	—	18	—	13	48	6	
	—	25	—	15	48	0	} Em. of I. at Tarentum.
	May	28	—	9	21	32	

The transit of Mercury over the Sun, in the year 1743, was observed at Naples, by Father P. N. with a telescope of 18 palms, made by Campani. He determined the first contact at $1^h 57' 25''$, and the second at $2^h 0' 35''$.

The transit of the same planet in 1753, was observed at Naples, in the Royal College, by Father Carcani, with a telescope of $18 \frac{1}{2}$ palms.

First contact at	23	5	51
Emerf. of center	23	7	28
Second contact	23	9	5

The transit of Venus over the Sun, of the year 1761, was observed at Naples, at the same place, by the same astronomer, with an excellent telescope of 24 palms.

		"		
The first contact	at	21	16	55
The second contact		21	35	20

The same transit was likewise observed at Malta, by several people. A serjeant in the marines, who is an excellent pilot, has posted himself at Valetta, and has an excellent clock, and a Newtonian reflector of 3 palms. He observed the beginning of the emerfion at $21^h 17' 50''$, and the total emerfion at $21^h 36' 33''$.

Finally, at Tarentum, the latitude of which place is the same with that of Naples, Mr. William Felton observed the transit of Mercury over the Sun of 1753, with a very good reflector of 2 feet.

		h	'	"
The first contact	at	11	18	26
The second contact		11	21	36

Eclipses of the three first Satellites of Jupiter, observed, at the Royal Observatory at Greenwich, in the years 1762, 1763, and 1764, during the time that the late Rev. Nathaniel Bliss, A. M. F. R. S. Professor of Geometry in the University of Oxford, was Astronomer Royal; communicated by Nevil Maskelyne, B. D. F. R. S. Astronomer Royal.

N. B. The observations were sometimes made with a 6 foot Newtonian reflector, the diameter of whose aperture is 9 inches; and which magnifies 100 times, and sometimes with a 2 foot reflector, the diameter of whose aperture is $4\frac{1}{2}$ inches, and which magnifies 90 times; and sometimes with another 2 foot reflector, made by Mr. Bird, whose aperture is 3,8 inches, which magnifies 88 times.

The 6 foot telescope shews an immersion of the first satellite later, and an emersion of the same sooner, than the first of the 2 foot telescopes by about 20". The difference between the two 2 foot telescopes may be supposed about 5".

	Aug.	26	at 12	51	45	} Imm. of I. satel. with 6 foot telescope.
	Sept.	18	— 13	9	29	
	Oct.	4	— 11	31	26	
1762	—	11	— 13	27	9	} Em. of I. with 6 foot telescope.
	Nov.	3	— 15	48	56	
	—	21	— 15	44	41 a little hazy	
	Dec.	23	— 4	57	40	

	Jan.	6	at	8	42	6	Em. of I. fatel. with 6 foot telefc.
	March	9	{	7	7	51	Em. of III. fatel. } 2 foot telescope,
				7	30	16	Em. of I. fatel. } N. Maskelyne.
1763	Sept.	30	—	13	7	52	Imm. of I. fatel. 6 foot
	Oct.	12	—	14	58	5	Imm. of II. fatel. }
	—	14	—	16	59	1	Imm. of I. fatel. }
		16	—	11	28	0	
	Feb.	24	—	12	29	42	Em. of I. fatel. }
	March	4	—	8	54	48	
	April	12	—	7	41	39	Imm. of I. fatel. 2 foot
1764	Sept.	25	—	14	31	49	
	Nov.	4	—	13	3	37	Imm. of I. fatel. }
	Nov.	10	{	14	57	11	
			{	14	56	57	2 foot }

Bird's 2 foot

The observations prior to the year 1762 fell in the time of Dr. Bradley's being Astronomer Royal, whose observations are not in my possession ; neither can I have access to them.

Nevil Maskelyne.

Received October 4, 1768.

XXXI. *An Account of some Experiments,*
by Mr. Miller of Cambridge, on the sow-
ing of Wheat: By W. Watson, M. D.
F. R. S.

To the Royal Society.

Gentlemen,

Lincoln's Inn Fields,
Oct. 4, 1768.

Read Nov. 24,
1768.

HAVING been informed, that in the Botanic garden at Cambridge, there had been produced, by the ingenuity and care of Mr. Charles Miller, the gardener there, from one grain of wheat only, in little more than a year, a much more considerable quantity of grain, than was ever attempted, or even conjectured to be possible; I have desired him to send me a particular account thereof, in order to its being communicated to you; and, if the Council should think proper, of its being recorded in the Philosophical Transactions, as I think it highly deserves. In my opinion, a fact so extraordinary should not be forgotten; as it may possibly be applied in no inconsiderable degree to public utility: if it should not, the experiment itself, so successfully conducted, is a desirable thing to be known.

Mr. Charles Miller is a very ingenious person, and an excellent naturalist. He is the son of our worthy brother

brother Mr. Philip Miller, from whose knowledge of, and publications in, botany, agriculture, and gardening, the public has received very great information and advantage. In consequence of my desire, Mr. Charles Miller has informed me, that having made, in the autumn of 1765, and in the spring of 1766, an experiment of the division and transplantation of wheat, by which near two thousand ears were produced from a single grain; and he having reason to think, from the success attending this experiment, that a much greater quantity might be produced, he determined to repeat the experiment next year.

Accordingly, on the second of June, 1766, he sowed some grains of the common red wheat; and, on the eighth of August, which was as soon as the plants were strong enough to admit of a division, a single plant was taken up, and was separated into eighteen parts. Each of these parts was planted again separately. These plants having pushed out several side shoots by about the middle of September, some of them were then taken up, and divided; and the rest of them between that time and the middle of October. This second division produced sixty seven plants.

These plants remained through the winter; and another division of them, made between the middle of March and the twelfth of April, produced five hundred plants. They were then divided no further, but permitted to remain.

The plants were in general stronger than any of the wheat in the fields. Some of them produced upwards of an hundred ears from a single root. Many of the ears
measured

measured seven inches in length, and contained between sixty and seventy grains.

The whole number of ears, which by the process beforementioned were produced from one grain of wheat, was twenty one thousand one hundred and nine, which yielded three pecks and three quarters of clear corn ; the weight of which was forty seven pounds, seven ounces; and, from a calculation made by counting the number of grains in one ounce, the whole number of grains might be about five hundred and seventy six thousand eight hundred and forty.

By this account we find, that there was only one general division of the plants made in the spring. Had a second been made, the number of plants, Mr Miller thinks, would have amounted, at least, to two thousand, instead of five hundred ; and the produce have been much enlarged. For he found by the experiment made the preceding year, in which the plants were divided twice in the spring, that they were not weakened by the second division. He mentions this to shew, that the experiment was not pushed to the utmost.

The ground, in which this experiment was made, is a light blackish soil upon a gravelly bottom, and consequently a bad soil for wheat. One half of the ground was very much dunged ; the other half was not prepared with dung, or any other manure : no difference was however discoverable in the vigour or growth of the plants, nor was there any in their produce.

Mr. Miller adds, that he omits making any conjectures of the probability of turning this experiment

to public utility in agriculture; as that, he hopes, may be better ascertained by a more extensive one, which he hopes to make next year. A gentleman, who assisted him in making the experiment last year, has sown half an acre of land with wheat, from which they expect to have sufficient to plant four acres next spring. The success of this experiment they propose to transmit to me, when it is completed; and of this, in due time, I shall not fail to inform you.

I am, Gentlemen,

Your most obedient

humble servant

W. Watson.

XXXII. *Of the Theory of Circulating Decimal Fractions.* By John Robertson, Lib. R. S.

Read Nov. 24, 1768. **T**HE great advantages arising from the use of Arithmetic, particularly in philosophy and commerce, is sufficiently known; therefore, every step taken towards its perfection, has always been countenanced by those who were best acquainted with its nature and value. On these motives I have been induced to offer the annexed paper to this learned Society.

Regiomontanus, it is said, first among Europeans, added to the then known arithmetic, an operation by decimal fractions; which he exemplified in his triangular table. Its utility was readily seen, and embraced in many nations, and particularly in this; where it appears to have been cultivated in its theory, and facile modes of operation, more than in other places.

Many writers have remarked its excellency in numeral computation, and have pointed out compendiums to avoid the trouble of writing down superfluous figures; particularly in the operations with concrete numbers, or those relative to money, weight, and measure; where the gradations from one denomination to another do not proceed in an uniform progression.

In finding the decimal values of the fractional parts of concrete, and other numbers, it often hap-

pens, that those decimals do not terminate, or end, with a few figures only; and sometimes are infinite, or never end; and among these are many which have one or more figures constantly recurring; as in the following proportions, *viz.*

$3 : 2 :: 1,0000, \text{ \&c.} : 0,6666, \text{ \&c.}$
 and $12 : 5 :: 1,0000, \text{ \&c.} : 0,4166, \text{ \&c.}$
 also $7 : 3 :: 1,0000, \text{ \&c.} : 0,428571,428571, \text{ \&c.}$

In operations, with such recurring decimal fractions, particularly in multiplication and division, the work will either be longer than necessary, or be very inaccurate, if the numbers are not considered as circulating ones: and to come at the true results of such operations, several authors have given precise rules; and some of them have shewn the principles upon which those rules were founded.

In the annexed paper those principles are, endeavoured to be, exhibited in a different, and in a more general and concise manner, than has hitherto been shewn: but the modes of working are not here annexed, as they are to be found in *Cunn*, *Malcolm*, *Marsh*, and others; and may hereafter be fully exemplified in a treatise of Arithmetic, by the author hereof, considered in a more mathematical order, than what has hitherto been appropriated to this most useful science.

GENERAL PRINCIPLES.

1. Number is supposed to begin at unity, and from thence to ascend and descend: those terms ascending above unity, are integers; and those descending below unity, are fractions.

When

When in the ascending and descending parts of the scale, the gradation proceeds by a tenfold value from the right hand towards the left, the rank of numbers, thus generated, is called the decimal scale.

As every place in this scale is ten times the value of its next right hand place; therefore the first place in the fractional part, is $\frac{1}{10}$ of the place of units; and the second, third, fourth, &c. descending places in the fractional part, is $\frac{1}{100}$, $\frac{1}{1000}$, $\frac{1}{10000}$, &c. part of the place of units.

Therefore every decimal fraction is equal to a series arising from multiplying the first, second, third, fourth, &c. terms of the decreasing geometrical progression $\frac{1}{10} + \frac{1}{100} + \frac{1}{1000} + \frac{1}{10000}$, &c. by the first, second, third, and fourth, &c. terms in the given fraction respectively.

Thus. Let the given fraction be $0,3587$ or $\frac{3587}{10000}$.

$$\begin{aligned} \text{Then } 0.3587 &= \frac{3}{10} + \frac{5}{100} + \frac{8}{1000} + \frac{7}{10000} \\ &= \frac{3}{10} + \frac{5}{100} + \frac{8}{1000} + \frac{7}{10000} \\ &= \frac{3000}{10000} + \frac{500}{10000} + \frac{80}{10000} + \frac{7}{10000} = \frac{3587}{10000} \end{aligned}$$

2. Every decimal fraction arises from division, when the dividend is less than the divisor.

For, divisor : dividend :: 10 : first term of the fraction ;

$$\therefore 100 : \text{sum of the first and second terms};$$

$\therefore 1000 : \text{sum of the first, second, and third, terms,}$
 $876.$

And according to the ratio of the divisor to the dividend, the quotient, or decimal fraction, will be finite or infinite.

3. Among those decimal fractions which are infinite, or do not end, some of them *recur*, or circulate; that is, the same figure or figures run over again and again *ad infinitum*.

As 0,333 မိငါး; 0,2323 မိငါး; 0,758758 မိငါး; 0,999 မိငါး.

Here $0,333 \text{ Gr.} = \frac{3}{10} + \frac{3}{100} + \frac{3}{1000} + \frac{3}{10000}, \text{ Gr.}$

$$0,2323 \text{ } \overline{23} \text{ } \textcircled{\scriptsize C}. = \frac{2}{10} + \frac{3}{100} + \frac{2}{1000} + \frac{3}{10000}, \text{ } \textcircled{\scriptsize C}.$$

0,785785 ဖြစ်. $= \frac{7}{10} + \frac{8}{100} + \frac{5}{1000} + \frac{7}{10000} + \frac{8}{100000} + \frac{5}{1000000}$ ဖြစ်.

$$0,999, \text{ \textit{E.c.}} = \frac{9}{10} + \frac{9}{100} + \frac{9}{1000} + \frac{9}{10000}, \text{ \textit{E.c.}}$$

circulating fraction 0,999, &c. is equal to 1,0.

The difference between 1,0 and 0,999 &c. is less than can be

be a repetition of figures, it is usual to mark the first and last of
g expressions, with points over the figures.

333 &c. is wrote 0,3̇

2323 &c. 0,2̇3̇

785785 &c. 0,7̇8̇5̇

These circulating decimal fractions are those which have each the same
of circulating places; and begin to recur each at the same name.

A finite decimal fraction may be considered as infinite; cyphers being
the circulating part.

The place of a circulating expression may be taken as the first; observ-
the number and order of the circulating places be not altered.

As the decimal fraction arises by division; if either place of the re-
figures be taken for the first, the others will from thence re-
circulate.

As several unlike circulating decimal fractions may be made to begin
at places of like names.

In the decimal scale 10, 100, 1000, 10000, 100000, 1000000, &c.
and indefinitely, be selected any rank of equi-distant terms, such,
whatever term therein is taken for the first term, and the first term is
the common ratio to the rest; then will the sum of the reciprocals of
terms, be equal to the reciprocal of the number which is unity less
the first term.

$$\frac{1}{9} + \frac{1}{99} + \frac{1}{999} + \text{\&c.} = \frac{1}{9}; 10 \text{ being the 1st term.}$$

$$\frac{1}{99} + \frac{1}{9999} + \frac{1}{999999} + \text{\&c.} = \frac{1}{99}; 100 \text{ being the 1st term.}$$

$$\frac{1}{999} + \frac{1}{999999} + \frac{1}{999999999} + \text{\&c.} = \frac{1}{999}; 1000 \text{ being the 1st term.}$$

For

$$\begin{aligned} \text{For } \frac{1}{10} + \frac{1}{100} + \frac{1}{1000} + \text{C.} &= \frac{100 + 10 + 1}{1000} = \frac{111 \text{ C.}}{1000 \text{ C.}} \\ \frac{1}{100} + \frac{1}{10000} + \frac{1}{1000000} + \text{C.} &= \frac{10000 + 100 + 1}{1000000} = \frac{10101 \text{ C.}}{1000000 \text{ C.}} \\ \frac{1}{1000} + \frac{1}{1000000} + \frac{1}{1000000000} + \text{C.} &= \frac{1000000 + 1000 + 1}{1000000000} = \frac{1001001 \text{ C.}}{1000000000 \text{ C.}} \end{aligned}$$

$$\begin{aligned} \text{But } 111 \text{ C.} \times 9 &= 1000 \text{ C. (by } 3^3) \text{ Then } \frac{111 \text{ C.}}{1000 \text{ C.}} = \frac{1}{9} \\ 10101 \text{ C.} \times 99 &= 100000 \text{ C.} \text{ Then } \frac{10101 \text{ C.}}{100000 \text{ C.}} = \frac{1}{99} \\ 1001001 \text{ C.} \times 999 &= 1000000000 \text{ C.} \text{ Then } \frac{1001001 \text{ C.}}{1000000000 \text{ C.}} = \frac{1}{999} \end{aligned}$$

Hence the reciprocal of a number consisting of n places of 9's, is equal to a circulating number of n places, the right hand figure being 1, and the rest 0's.

$$\begin{aligned} \text{Thus, } \frac{1}{9} &= \frac{1}{1000 \text{ C.}} \\ &= \frac{10101 \text{ C.}}{1000000 \text{ C.}} = 0,0\dot{1} \\ &= \frac{1001001 \text{ C.}}{1000000000 \text{ C.}} = 0,00\dot{1} \\ &= \frac{10001001 \text{ C.}}{1000000000000 \text{ C.}} = 0,000\dot{1} \end{aligned}$$

6. If the reciprocal of a number consisting of n places of 9's, be multiplied by a number D , not exceeding n places; the product will be a circulating decimal fraction of n places, the right hand ones being the same digits as are in the number D .

Let $D = 3$; or $D = 23$; or $D = 785$; or to any other number.

$$\frac{1}{9} = \frac{111 \text{ ȳ.}}{1000 \text{ ȳ.}}; \quad \frac{1}{99} = \frac{10101 \text{ ȳ.}}{1000000 \text{ ȳ.}}; \quad \frac{1}{999} = \frac{1001001 \text{ ȳ.}}{1000000000 \text{ ȳ.}} \quad (\text{by 5th})$$

$$\text{ore } \frac{1}{9}, \text{ or } \frac{111 \text{ ȳ.}}{1000 \text{ ȳ.}} \quad \times 3 = \frac{333 \text{ ȳ.}}{1000 \text{ ȳ.}} = 0, \dot{3}$$

$$\frac{1}{99}, \text{ or } \frac{10101 \text{ ȳ.}}{1000000 \text{ ȳ.}} \quad \left\{ \begin{array}{l} \times 3 = \frac{30303 \text{ ȳ.}}{1000000 \text{ ȳ.}} = 0, \dot{0}\dot{3} \\ \times 23 = \frac{232323 \text{ ȳ.}}{1000000 \text{ ȳ.}} = 0, 2\dot{3} \end{array} \right.$$

$$\frac{1}{999}, \text{ or } \frac{1001001 \text{ ȳ.}}{1000000000 \text{ ȳ.}} \quad \left\{ \begin{array}{l} \times 3 = \frac{3003003 \text{ ȳ.}}{1000000000 \text{ ȳ.}} = 0, \dot{0}\dot{0}\dot{3} \\ \times 23 = \frac{23023023 \text{ ȳ.}}{1000000000 \text{ ȳ.}} = 0, \dot{0}\dot{2}\dot{3} \\ \times 785 = \frac{785785785 \text{ ȳ.}}{1000000000 \text{ ȳ.}} = 0, 7\dot{8}\dot{5} \end{array} \right.$$

ence every circulating decimal fraction will be equivalent to a vulgar fraction wherein the numerator is those circulating figures, and the denominator consists of as many 9's, as are figures in the numerator.

$$0, \dot{3} = \frac{3}{9}. \quad \text{For } \frac{1}{9} \times 3 = \frac{3}{9} = 0, \dot{3} \quad (\text{by 6th})$$

$$0, \dot{0}\dot{3} = \frac{3}{99} \quad \frac{1}{99} \times 3 = \frac{3}{99} = 0, \dot{0}\dot{3}$$

$$0, 2\dot{3} = \frac{23}{99} \quad \frac{1}{99} \times 23 = \frac{23}{99} = 0, 2\dot{3}$$

ence a circulating decimal fraction, of any number of places, being multiplied by a number of as many 9's, will give a finite expression, having as many figures as are in the circulating one.

$$\text{hus } 0, \dot{6} \times 9 = 6. \quad \text{For } 9 : 1 :: 6 : 0, \dot{6}$$

$$0, \dot{0}\dot{6} \times 99 = 6. \quad 99 : 1 :: 6 : 0, \dot{0}\dot{6}$$

$$0, 2\dot{5} \times 99 = 25. \quad 99 : 1 :: 25 : 0, 2\dot{5}$$

$$0, 62\dot{5} \times 999 = 625. \quad 999 : 1 :: 625 : 0, 62\dot{5}$$

Hence

Hence it appears, that, in common multiplication, the product of a circulating number, by its proper denominator, in 9's, will be deficient of the true product by that circulating number.

Thus $\dot{0},\dot{6} \times 9 = 5,4$; then $5,4 + ,6 = 6$. For $\frac{6}{9} = 0,\dot{6}$

$0,\dot{0}\dot{6} \times 99 = 5,94$; then $5,94 + 0,06 = 6$. For $\frac{6}{99} = 0,0\dot{6}$

$0,6\dot{2}\dot{5} \times 999 = 624,375$; then $624,375 + 0,625 = 625,0$

Hence. Any finite number is in proportion to the same number recurring, as the proper denominator of the circulate is to that denominator increased by unity.

Thus $9 : 10 :: 6 : \dot{6}$. For $\dot{6} \times 9 = 6 \times 10$

$99 : 100 :: 25 : \dot{2}\dot{5}$. For $\dot{2}\dot{5} \times 99 = 25 \times 100$.

SCHOLIUM.

If to the preceding articles, be joined the compendiums of multiplying and dividing by any number of 9's, they will constitute the whole of the theory, upon which depend all the operations with circulating numbers: for as these have 9's for their denominator, wanting unity in the lowest place to make them 10's; therefore unity for every 9 is applied in some additions and multiplications: Or, the circulating parts being reduced to finite number; then working with them by the common rules, will give finite results; which results are to be reduced to circulates by contrary operations to what were used to reduce the circulates to finites.

XXXIII. *A Letter from Mr. J. R. Forster, F. A. S. to M. Maty, M. D. Sec. R. S. containing some Account of a new Map of the River Volga.*

Warrington, October 24, 1768.

S I R,

Read Nov. 8, 1768. **I** Received both your favours of the 11th and 17th instant, in due time.

I am very glad the committee of the Royal Society has done my map of the river Volga * the honour of having it engraved and published in their Transactions.

You wished to be informed of the manner in which it was constructed. I readily comply with your desire, and send you the following short account, which I hope will be satisfactory.

At my arrival at Saratof, which is the chief town of the district given by her Russian majesty to the German colonies, I got two MSS. maps in Russian characters, done by Mr. Reufs, a major, and able engineer, in the Russian service, who went along the river Volga, from Saratof to Tšaritsin, by water; and, at every winding of the river, went on shore to take the angles, and to measure the meadows beyond the Volga; this map was upon five sheets, pasted together. The other MS. map had the same author, consisted of eight sheets, and described the country from Petrofsk and Saratof, to Tšaritsin, along the Volga, and westward to the rivers Choper and Don. It was constructed upon an actual survey, in which the major was assisted by three engineer officers; however,

* See TAB. IX. p. 216.

as this survey had been made in a hurry, by measuring the roads on horseback, with a long rope of ten poles, and taking the angles only from village to village, I cannot depend upon it as entirely correct.

The country from Saratof to Dmitrefsk I passed through myself; I measured one base, and then took, with an instrument, the angles of the most striking objects, as hills, villages, and rivers, on both sides of the Volga. On the eastern side of that river, I got as far as the lake Yelton, and the sandy desert Ryn, always using the same method: from thence I went to the river Yerooflan, almost to its very source, and came back again, to the place where it enters the Volga; from thence I proceeded to Saratof, along the Volga, rectifying Mr. Reufs's angles and maps; and, besides, I made an excursion 30 miles above Saratof, on the east shore of the Volga. All the places, which are marked with lines in my map, are either colonies already settled, or places intended to be filled with colonists, who were then on their march from Petersburgh towards Saratof. When I presented this map to the Academy of Sciences at Petersburg, it was looked upon as an Unique; for they had in the geographic department not one map of this country, although more than 3000 MSS. maps and drawings were kept in their portefeuilles. I can, therefore, very justly call this the first tolerable map; and whoever will take the trouble of comparing my work with Mr. Hanway's, or Olearius's map, will easily see the remarkable difference between them. Mr. Hanway went in a great hurry over this part of the country, and had no instruments, nor did he make any survey; he was also unacquainted with the language of the country; the

the knowledge of which is of great importance to one who would succeed in an undertaking of this nature. Olearius never stirred out of his ship: and the map contained in the Russian Atlas, is so shamefully deficient and faulty, that the academy very readily acknowledged the improvements made by my map, and thankfully accepted of it, although no use was made of it. During my stay at London, in 1766, I made a revision of my papers, calculated again all distances and angles, and corrected my map in more than twenty places, so that this may be justly called a new and improved one.

The canal made by Perry, in the year 1697-1701, for the conjunction of the Volga and Don, I surveyed; and constructed a special map of it, of more than three yards size, and can therefore answer for its accuracy. The canal begun by the Turks, nobody ever took notice of besides me in a map; which I did, from an account I got, by means of Mr. Reufs, and some others, and from a rough sketch of it, communicated to me. That part of the Don, which appears in the map, is taken from the very accurate and famous map made by the vice-admiral Cornelius Cruys, and published in fourteen sheets in Holland.

This, I hope, will satisfy the public in respect to the accuracy of the performance, and enable the Society to judge better, whether it deserves to be published or not. I am, with the greatest respect,

S I R,

Your most obliged and humble servant,

John Reinhold Forster.

Received October 3, 1768.

XXXIV. *An Account of the Lymphatic System in Birds; by Mr. William Hewson, Reader in Anatomy: In a Letter to William Hunter, M. D. F. R. S. and by him communicated to the Society.*

S I R,

Read December 8, 1768. **H**AVING been so fortunate, in a

series of experiments made with that view, as to trace out the lymphatic system in birds, I have ventured to offer the following account of it to you, in order to be presented, if you think proper, to the Royal Society; and, I flatter myself, this discovery will be looked upon as some acquisition to physiology.

The lymphatic system has been supposed to be wanting in birds; and absorption in that kind of animals to be carried on by branches of the common veins. Physiologists were led into this opinion by observing, that though the lacteals and mesenteric glands were easily seen even in the smallest quadruped, yet the most acute anatomists had not been able to find in any bird the least appearance either of those vessels or glands. The difficulty of discovering the lacteals in birds was, no doubt, principally owing to the transparency, or want of colour, in the fluid which they contain. In quadrupeds the lacteals are easily found, as they are filled with chyle, which is mostly opaque and white; whereas, in birds, the chyle is as pellucid and colourless as the vessels themselves.

The want of mesenteric glands was another cause of our remaining so long ignorant of those vessels.

This system may be divided in birds, as it is in quadrupeds, into the branches, viz. the lacteals and lymphatics, and their trunk, or thoracic duct. The lacteals indeed, in the strictest sense, are, in birds, the lymphatics of the intestines, and like the other lymphatics carry a transparent lymph. And instead of one thoracic duct there are two, of which one goes to each jugular vein. In these circumstances it would seem, that birds differ from quadrupeds, so far at least as I may judge from the dissection of a goose, which was the bird I chose as most proper for this enquiry.

So much being premised, I shall next give a description of what I have seen of those vessels in this fowl; and to illustrate the description I shall add a figure from the same subject, in which those vessels were filled with quick-silver.

The lacteals run from the intestines upon the mesenteric vessels. Those of the *duodenum* (*a*)* pass by the side of the *pancreas* (*Q*) †, and probably receive its lymphatics: afterwards they get upon the cœliac artery, of which the superior mesenteric is a branch. Whilst they are upon this artery they are joined by the lymphatics from the liver (*b*): here they form a plexus, which surrounds the cœliac artery (*cc*); at this part they receive a lymphatic from the gizzard (*d*); and a little farther, another from the lower or glandular part of the *œsophagus* (*e*). Having now got to the root of the cœliac artery, they are joined by the lymphatics from the renal *capsule*; and near the same part, by the

* See Plate X. in the outlines.

† See the same Plate in the Figure itself.



laeteals from the other small intestines, which vessels accompany the lower mesenteric artery. These last mentioned laeteals, before they join those from the *duodenum*, receive from the *rectum* a lymphatic, which runs with the blood-vessels of that gut. Into this lymphatic some small branches from the kidneys seem to enter, which coming from those glands upon the mesentery of the *rectum*, at last open into its lymphatics. At the root of the coeliac artery, the lymphatics of the lower extremities probably join those from the intestines. The former I have not yet traced to their termination, though I have distinctly seen them on the blood-vessels of the thigh; and in one subject, which I injected, some vessels were filled, contrary to the course of the lymph, from the network near the root of the coeliac artery; these vessels ran behind the *cava*, and down upon the *aorta*, near to the origin of the crural arteries, and I presume they were the trunks of those branches which I had seen in the thigh. At the root of the coeliac artery, and upon the contiguous part of the *aorta*, a network (*ff*) is formed by the laeteals and lymphatics above described. This network consists of three or four transverse branches, which make a communication between those which are lateral. In the subject from which my drawing was made, there were four. From this network arise the two thoracic ducts (*gg*); of which one lies on each side of the spine, and runs upon the lungs obliquely upwards towards the jugular vein, into which it opens (*l & n*); not indeed into the angle between the jugular and subclavian, as in the human subject, but into the inside of the jugular vein, nearly opposite to that angle. The thoracic duct of the left side is joined

by a large lymphatic (*b*), which runs upon the *oesophagus*, and can be traced as far as the lower or glandular part of that canal; from which part, or from the gizzard, it seems to issue. The thoracic ducts are joined by the lymphatics of the neck (and probably by those of the wings) just where they open into the jugular veins.

The lymphatics of the neck * generally consist of two pretty large branches, on each side of the neck, accompanying the blood-vessels. Those two branches join near the lower part of the neck; and the trunk is, in general, as small, if not smaller, than either of the branches. This trunk runs close to the jugular vein (*ii*), gets on its inside, and then opens into a lymphatic gland (*k k*). From the opposite side of this gland, a lymphatic comes out, which pours the lymph into the jugular vein. On the left side, the whole of this lymphatic joins the thoracic duct of the same side, (*l*); but, on the right, one part of it goes into the inside of the jugular vein a little above the angle (*m*), whilst another joins the thoracic duct, and with that duct forms a common trunk, which opens into the inside of the jugular vein, a little below the angle which that vein makes with the subclavian (*n*).

To this description it may be necessary to add, that though it be taken from one subject, yet in three others of the same species which I examined carefully, I saw nothing which disagreed with it. I particularly attended to the number of the thoracic ducts, suspecting, that possibly in this subject, the two that I had seen might

* It is but doing justice to the ingenious Mr. John Hunter, to mention here, that these lymphatics in the necks of fowls were first discovered by him many years ago.

be only a variety, which is a circumstance that, as we are told, has occurred even in the human body. But in three others of this species, which I likewise successfully injected, I still saw two ducts; and therefore I am inclined to believe, that this is the constant number. I likewise carefully attended to the vessels coming from the gland on the right side: and in the only two subjects in which the lymphatics of the neck were properly filled, I observed, that one part of it opened immediately into the vein, and the other joined the thoracic duct of that side; whilst, on the left side, the vessel which issued from the gland wholly joined the thoracic duct. In all the four subjects I evidently saw that the thoracic ducts open into the inside of the jugular veins.

This system in birds differs most from that in quadrupeds in the following particulars. 1st, In the chyle being transparent and colourless. 2dly, In there being no visible lymphatic glands, neither in the course of the lacteals, nor in that of the lymphatics of the *abdomen*, nor near the thoracic ducts. 3dly, In the several parts of this system in birds being more frequently enlarged, or varicose, than in quadrupeds. In particular, this appears to be the case of the vessels which constitute the network at the root of the cœliac artery in that subject from which the drawing was taken. The lacteals are frequently enlarged in some places; so are the thoracic ducts; and the lymphatics on each side of the neck are commonly, when taken together, larger than their trunk which opens into the lymphatic gland. In one subject, where instead of two lymphatics on the left side I found only one, that vessel was as large as a crow-quill; whilst the
lower

lower part of it, which entered the gland, was much smaller.

Thus far the account of what I saw : I shall next beg leave to observe, that, as the supposed want of this system in birds has been considered as a strong argument in favour of absorption by the common veins, now, since we find it not wanting, that theory must be much weakened. And I may likewise add, that absorption seems to be carried on in birds, as in quadrupeds, by this system, at least principally ; indeed I am inclined to believe, entirely ; for no arguments brought in favour of absorption by the common veins appear to me of equal validity with those that can be urged against it. The contrary opinion is indeed embraced by the most learned and acute physiologist of the present age, who, treating of this subject, expresses himself in the following manner ;

“ It is a strong argument in favour of absorption by
 “ the common veins, that neither birds, amphibious
 “ animals, nor fish with cold blood, have either the
 “ lacteal or the lymphatic system. Nature common-
 “ ly observes a pretty strict analogy in her works, and
 “ makes use of similar organs to perform similar func-
 “ tions. Now in all animals, quadrupeds and the
 “ whale excepted, we must admit of absorption by
 “ the mesenteric veins, if in those animals there is
 “ no other way for the chyle to get into the blood.
 “ And if those veins in birds and amphibious animals
 “ absorb the chyle, it is very probable they likewise
 “ absorb it in quadrupeds, in which they equally
 “ exist.” But the existence of this system in birds
 is not the only fact which might be adduced to in-
 validate the above opinion ; for I have seen a part of it
 very distinctly in one of the *amphibia*, viz. the
 Turtle

Turtle *. Whether it is to be found in fish, I cannot yet determine. Since I saw it in birds and in the Turtle, I made indeed some enquiries after it in fish, but hitherto without success. Yet, that they are not without such vessels, I think is probable, from considering that the lymphatics are so general, as to be found in quadrupeds, birds, and amphibious animals. And from the consideration of the extensiveness of this system through so many classes of animals, I am inclined to think that opinion most probable, which you advanced some time ago, when you printed your discovery of the use of those vessels, viz. "That the lymphatics are the *only* absorbents †".

For the sake of those who may incline to prosecute this enquiry farther, I shall now relate the method by which these vessels may be demonstrated; and that is, having chosen a young and very lean goose, and fixed it upon a table, let the abdomen be opened whilst it is yet alive, and a ligature be passed round its mesenteric vessels, as near the root of the mesentery as possible. The lacteals will begin to appear near the ligature in

* The part of this system, which I saw in the Turtle, was the lacteals. I filled them with quick-silver as far as the root of the mesentery, where they formed a considerable net-work into which a lymphatic of the spleen entered. I had not an opportunity of tracing them farther, having taken the mesentery out of the animal before I had thought of looking for these vessels, as I was not at that time intent on this enquiry. The lacteals in that animal agreed with those in the bird above described, in not having any mesenteric glands. From this circumstance, and from another observation which I made, I am inclined to believe, that the whole system in this animal will be found to agree pretty exactly with that of birds.

These vessels I observed so long ago as in the winter 1763-64.

† Vide Hunter's Commentaries, ch. v.

a few minutes after it is made, especially if the bird has been well fed three or four hours before the experiment. The lymphatics in the neck may be shewn in the same manner; that is, by making a ligature on the jugular vein at the lower part of the neck; and to be more certain of including the lymphatics, which are near it, we must take care not to pass the needle too close to that vessel. When they are to be injected, they must be opened at a convenient part, and a proper pipe fixed in them for that purpose.

For the greater satisfaction of those who may think this paper worthy their attention, I have prepared two birds, whose lymphatic systems are filled with quick-silver, in order to be compared with the figure: these have already been shewn to several members of the learned Society, who honoured me with their presence whilst the subjects were fresh; and who, I flatter myself, were then satisfied with the exactness of the drawing.

Mr. Hewson begs leave to add, that since the above paper on the lymphatic system in birds was put into the hands of the secretary of the Royal Society, he has discovered the same system in fish; and has likewise been so fortunate as to procure a Turtle, whose lymphatic system he has traced out, and has got delineated. An account of those dissections, with the figures, he intends soon to have the honour of laying before the Society.

Windmill Street, Dec. 3,
1768.

Expla-

Explanation of P L A T E X.

N. B. *The small Letters refer to the Outlines, and the Capital Letters in general refer to the Figure, except where the contrary is specified.*

- A The Neck.
- BB The Clavicle divided near its middle.
- C The left Subclavian Artery.
- DD The Jugular Veins.—See the Outlines.
- EE The Pulmonary Arteries.
- FF The two Branches of the Trachea.
- GG The Lungs.
- H The Aorta—in the Outlines.
- I The Cœliac Artery—in the same.
- L The Oesophagus turned to a side.
- MM The Renal *Capfulæ*, or *Glandulæ Renales* — in the Outlines.
- N A small part of the Liver, fixed to a Rib by a thread.—In the Outlines.
- OOO Intestines.
- P The *Duodenum*.
- Q The *Pancreas* fixed to a Rib by a thread.
- R The Gizzard.

- a The Lacteals which come from the *Duodenum*.
- b The Lymphatics of the Liver.
- cc A *Plexus* formed by the above mentioned Lacteals and Lymphatics, which surrounds the Cœliac Artery.
- d A Lymphatic from the Gizzard.

- c A Lymphatic from the lower part of the *Oesophagus*.
- ff A Network formed by the Lymphatics upon the *Aorta*.
- gg The two Thoracic Ducts.
- ii The Trunks of the Lymphatics of the Neck.
- kk The Glands through which the Lymphatic Vessels of the Neck pass. That of the left side is oblong, and could not well be represented in a Figure.
- l The Thoracic Duct of the Left Side, and the Lymphatic Vessel of the Neck, opening together into the inside of the Jugular Vein.
- m A part of the Lymphatic vessel of the right side of the neck, opening into the Jugular Vein.
- n The Thoracic Duct of the right side, joined by a part of the Lymphatic Vessel of the neck, and then opening into the inside of the Jugular Vein.

XXXV. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1767, pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. et Soc. Reg. nuper Præses: By William Hudson, Societatis Regiæ & clariss. Societatis Pharmaceut. Lond. Soc. Hort. Chelsean. Præfectus et Prælector Botanici.*

Read Dec. 8, 2251
1768.

A *Lcea ficifolia*, foliis palmatis.
Lin. Sp. pl. 967.

Malvea rosea, folio ficus. Bauh. pin. 315.

2252 *Allium carinatum*, caule planifolia bulbifera,
staminibus subulatis. Lin. Sp. pl. 426.

Allium umbella bulbifera, vagina bicorni, foliis
carinatis. Hall. all. 27. tab. I. f. 2.

2253 *Amaranthus spinosus*, racemis pentandricis erectis,
axillis spinosis. Lin. Sp. pl. 1407.

Amaranthus Indicus spinosus, spica herbacea.
Herm. Lugdb. 31. tab. 33.

2254 *Bidens bipinnata*, foliis bipinnatis incisis, caly-
cibus involucreatis, corollis semiradiatis, semi-
nibus divergentibus. Lin. Sp. pl. 1166.

Chrysanthemum Americanum, coridis indi folio.
Herm. Par. 123. tab. 123.

G g 2

2255 *Bidens*

- 2255 *Bidens pilosa*, foliis pinnatis subpilosis, caulis geniculis barbatis, calycibus involucri simplicis, seminibus divergentibus. Lin. Sp. pl. 1166.
Bidens latifolia, hirsutior, semine angustiore radiato. Dill. Hort. Elth. 51. tab. 43. f. 51.
- 2256 *Campanula*, *Erinus*, caule dichotomo, foliis sessilibus, utrinque dentatis, floralibus oppositis. Lin. Sp. pl. 240.
Erini f. *Rapunculi* minimum genus. Column. phytob. 122. tab. 28.
- 2257 *Capparis spinosa*, pedunculis solitariis unifloris, stipulis spinosis, foliis annuis, capsulis ovalibus. Lin. Sp. pl. 720.
Capparis spinosa, fructu minore, folio rotundo. Bauh. pin. 480.
- 2258 *Conyza hirsuta*, foliis ovalibus, integerrimis scabris subtus hirsutis. Lin. Sp. pl. 1209.
- 2259 *Coreopsis lanceolata*, foliis lanceolatis integerrimis ciliatis. Lin. Sp. pl. 1283.
Bidens succisæ folio, radio amplo laciniato. Dill. H. Elth. 55. tab. 48. f. 56.
- 2260 *Coreopsis leucantha*, foliis pinnatis serratis, florum radio diversicolore. Lin. Sp. pl. 1282.
- 2261 *Dianthus glaucus*, floribus subsolitariis, squamis calycinis lanceolatis quaternis brevibus, corollis crenatis. Lin. Sp. pl. 588. Hudf. Fl. Angl. 161.
- 2262 *Dianthus Carthusianorum*, floribus subaggregatis: squamis calycinis ovatis aristatis tubum subæquantibus, foliis sublinearibus trinerviis. Lin. Sp. pl. 586.

- Caryophyllus sylvestris vulgaris latifolius. Bauh.
pin. 209.
- 2263 Fraxinus, *Ornus*, foliolis serratis, floribus corol-
latis. Lin. Sp. pl. 1510.
Fraxinus florifera botryoides, Morif. præl. 265.
Hort. Angl. 33. tab. 9.
- 2264 Forskohlea. Lin. Mantif. 72.
Chamædryfolia tomentosa mascatenfis. Pluk.
alm. 97. tab. 275. f. 6.
- 2265 Geropogon *glabrum*, foliis glabris. Lin. Sp. pl.
1109.
Tragopogon gramineo folio, glabrum, flore di-
lute incarnato. Raj. Hist. 111. 149.
- 2266 Gnaphalium *undulatum*, herbaceum foliis de-
currentibus, lanceolatis, acutis undatis subtus
tomentosis. Lin. Sp. pl. 1197.
Elichrysum graveolens acutifolium, caule alato.
Dill. Hort. Elth. 130. tab. 108. f. 130.
- 2267 Hamellia *patens*, racemis patentibus. Lin. Sp.
pl. 246. Jacq. Amer. 16.
Periclymenum arborefcens, ramulis inflexis;
flore luteo. Plum. ic 218.
- 2268 Helenium *autumnale*. Lin. Sp. pl. 1248.
After floridianus, caule alato. Pluk. Phyt. tab.
372. f. 9.
- 2269 Helicteres, *Ifora*, foliis cordatis serratis, fructu
composito contorto. Lin. Sp. pl. 1367.
Hicteres, arbor Indiæ orientalis, filiqua varico-
sa, et funiculi in modum contortuplicata.
Pluk. Alm. 181. tab. 245. f. 2.
- 2270 Holosteum *cordatum*, foliis subcordatis. Aman.
ac. 3. p. 21. Lin. Sp. pl. 130.

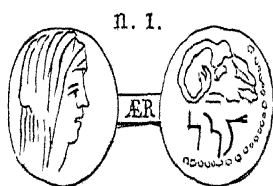
- Alfine Americana, nummulariæ folio. Herm.
parad. 11. tab. 11.
- 2271 *Holosteum umbellatum*, floribus umbellatis.
Loef. it. 120. Lin. Sp. pl. 130.
Lychnis graminea hirsuta umbellifera. Morif.
Hist. 2. p. 546. f. 5. tab. 22. f. 46.
- 2272 *Hypericum tomentosum*, floribus trigynis: caly-
cibus ferrato-glandulosis, foliis semiamplexi-
caulibus flexuosis tomentosis, caulibus pro-
stratis. Gouan. monsp. 402. Lin. Sp. pl.
1106.
Hypericum supinum tomentosum alterum.
Clus. Hist. 2. p. 181.
- 2273 *Lobelia, Erinus*, caule patulo, foliis lanceo-
latis ferratis, pedunculis longissimis. Lin. Sp.
pl. 1320.
Campanula minor Africana, crinifacie, flore
violaceo, caulibus erectis. Herm. Lugdb. 110.
tab. 111.
- 2274 *Loeflingia, Hispanica*, Loef. it. 113. tab. 1. f. 2.
Lin. Sp. pl. 50.
- 2275 *Malva scabrosa*, caule fruticoso, pilis simplicibus,
foliis lobatis, floribus erectiusculis, petalis
incumbentibus. Lin. Sp. pl. 968.
Malva Africana frutescens, flore rubro. Comm.
hort. 2. p. 171. tab. 86.
- 2276 *Malva limensis*, caule erecto herbaceo, foliis
lobatis, spicis secundis axillaribus, seminibus
lævibus. Lin. Sp. pl. 968.
- 2277 *Mimulus ringens*, erectus, foliis oblongis linea-
ribus sessilibus. Lin. Sp. pl. 884.
Digitalis perfoliata glabra, flore violaceo minore.
Hist. Ox. 11. p. 475. f. 5. tab. 8. f. 6.
- 2278 *Morus*

- 2278 *Morus papyrifera*, foliis palmatis, fructibus hispidis. Lin. Sp. pl. 1399.
Morus papyrifera, fativa Japonica. Seb. Thes. 1. p. 44. tab. 28. f. 3.
- 2279 *Parietaria Lusitanica*, foliis ovatis obtusis, caulibus striatis lævibus filiformibus procumbentibus. Lin. Sp. pl. 1492.
Parietaria Lusitanica, annua minima. Tourn. 506. Raj. Hist. III. p. 129.
- 2280 *Passiflora rubra*, foliis bilobatis cordatis acuminatis. Lin. Sp. pl. 1356.
Flos passionis, folii media lacinia quasi abscissa, flore minore carneo. Sloan. Jam. 144. Hist. I. p. 229.
- 2281 *Passiflora serratifolia*, foliis indivisis serratis. Lin. Sp. pl. 1355.
Granadilla Americana, folio oblongo leviter serrato, petalis ex viridi rubescentibus. Mart. cent. 36. tab. 36.
- 2282 *Plantago maritima*, foliis semicylindræis integerrimis : basi lanatis, scapo tereti. Lin. Sp. pl. 165. Hudf. Angl.
Coronopus maritimus major. Bauh. pin. 190.
- 2283 *Plantago Africana*, caule ramoso fruticoso, foliis lanceolatis, dentatis capitulis aphyllis. Lin. Sp. pl. 168.
Psyllium, foliis crenatis, Indicum. Bauh. pin. 191.
- 2284 *Plantago Cynops*, caule ramoso fruticoso, foliis filiformibus integerrimis strictis, capitulis subfoliatis. Lin. Sp. pl. 167.
Psyllium majus supinum. Bauh. pin. 191.
- 2285 Poly-

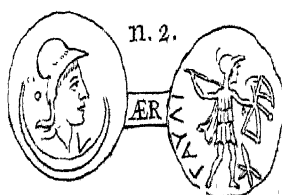
- 2285 Polycarpon *tetraphyllum*. Lin. Sp. pl. 131.
 Anthyllis alfinifolia polygonoides majo. Barr.
 car. 103. tab. 534.
- 2286 Piercea *glabra*, foliis ovato-lanceolatis glabris.
 Mill Dict. Hort. ed. 8.
 Rivina, floribus octandris dodecandrisve. Lin.
 Sp. pl. 117.
- 2287 Saxifraga *umbrosa*, foliis obovatis subretusis cartilagineo-crenatis, caule nudo paniculato.
 Lin. Sp. pl. 574.
 Geum folio subrotundo minori pistillo floris
 rubro Mill. i. e. 141. f. 2.
- 2288 Sida *Indica*, foliis cordatis sublobatis stipulis
 reflexis, pedunculis longioribus, capsulis
 multilocularibus scabris calyce longioribus.
 Lin. Sp. pl. 964.
 Abutilon Indicum. Comm. hort. 11. tab. 1.
- 2289 Silphium *perfoliatum*, foliis oppositis deltoidibus, petiolatis perfoliatis. Lin. Sp. pl. 1301.
- 2290 Silene *polyphylla*, foliis fasciculatis setaceis: ramorum florentium oppositis. Roy. Lugdb. 447.
 Lin. Sp. pl. 601.
 Lychnis sylvestris VIII. Clus. Hist. I. p. 290.
- 2291 Sifymbrium *monense*, acaule, foliis dentatopinnatis subpilosis. Lin. Sp. pl. 118. Hudf.
 Angl.
 Eruca monensis laciniata, flore luteo majore. Dill. Hort. Elth. 135. tab. 111.
 f. 135.
- 2292 Solanum *ferox*, caule aculeato herbaceo, foliis cordatis angulatis tomentosis, baccis hirtis calyce obtectis. Lin. Sp. pl. 267.

- 2293 *Solanum Virginianum*, caule aculeato herbaceo
foliis pinnatifidis utrinque aculeatis: laciniis,
sinuatis obtusis, calycibus aculeatis. Lin. Sp.
pl. 267.
Solanum Americanum laciniatum spinosissimum. Dill. Hort. Elth. 360. tab. 267.
f. 346.
- 2294 *Solanum foderneum*, caule aculeato fruticoso,
foliis obovatis pinnatifido-sinuatis obtusis
sparsè aculeatis nudis, calycibus aculeatis.
Lin. Sp. pl. 265.
Solanum spinosum, profunde laciniatis foliis,
subtus lanuginosis, madaraspatanum. Pluk.
tab. 316. f. 4?
- 2295 *Solidago rigida*, foliis caulinis ovatis scabris,
ramis alternis fastigiatis, corymbis termina-
libus. Lin. Sp. pl. 1235.
Virga aurea novæ Angliæ, lato rigidoque folio.
Herm. par. 243. tab. 243.
- 2296 *Sonchus maritimus*, pedunculo nudo, foliis
lanceolatis amplexicaulibus indivisis retror-
sum argute dentatis. Lin. Sp. pl. 1116.
Sonchus angustifolius maritimus. Pluk. phyt.
tab. 62. f. 5.
- 2297 *Spermacoce tenuior*, glabra, foliis linearibus,
staminibus inclusis. Lin. Sp. pl. 147.
Spermacoce verticillis tenuioribus. Dill. Hort.
Elth. 370. tab. 277. f. 359.
- 2298 *Urtica cylindrica*, foliis oppositis oblongis, amen-
tis cylindricis solitariis indivisis sessilibus. Lin.
Sp. pl. 1396.
Urtica racemosa humilior iners. Sloan. Jam.

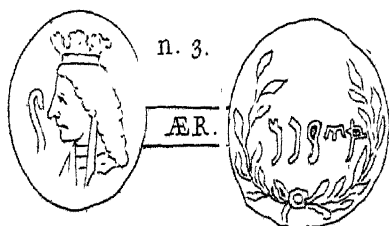
- 2299 *Urtica divaricata*, foliis alternis ovatis, racemis compositis divaricatis. Lin. Sp. pl. 1397.
Urtica racemosa major Virginiana mitior, f. minus urens. Pluk. phyt. 237. f. 2.
- 2300 *Zinnia pauciflora*, floribus paucis. Lin. Sp. pl. 1296.
Bidens calyce oblongo, feminibus radii corolla non decidua coronatis. Mill. tab. 64.



Penes Joannem Swinton, S.T.B.
Oxonienf. R. S. S.



Apud Joannem Swinton, S.T.B.
Oxonienf. R. S. S.



Apud Car. Godwyn, S.T.B.
Coll. Ball. Oxon. Soc.



Apud Joannem Swinton, S.T.B.
Oxonienf. R. S. S.

Received September 29, 1768.

XXXVI. *Interpretation of the Inscription on a Punic Coin, struck in the Isle of Gozo, never hitherto explained. In a Letter to Charles Morton, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read Nov. 24, 1768. **I**T has been observed by the learned Sig. Abate Ridolfino Venuti, that such antient pieces as that before me (see TAB. XI. n. 5.), which has a place in my small cabinet, adorned with a Punic inscription on the reverse (1), are not seldom found in the island of Malta. This seems in a good measure to have induced him to denominate them Maltese (2) medals, and to consider them as struck in that island. On one side of my coin we disco-

(1) *Saggi di Dissertazioni Accademiche pubblicamente lette nella Nobil. Accademia Etrusca dell' antichissima Città di Cortona.* Tom. I. p. 35—42.

(2) *Dissertaz. dell' Abat. Ridolfin. Venut. in Sag. di Dissertaz. Accademiche.* &c. ubi sup.

ver the head of a woman veiled, and on the reverse that of a sheep, under which stands a word formed of three Punic characters. The piece itself is well enough preserved, and the letters in particular have almost intirely escaped the injuries of time. Several similar medals have been published by (3) Paruta, (4) Laftanofa, the (5) Marquis Scipio Maffei, (6) Sig. Abate Venuti, and (7) M. Pellerin; on which the three elements here mentioned, though, perhaps, not fo accurately taken as thofe transmitted you in the draught of my coin, are exhibited to our view. The legend or infcription, however, on the reverse has never, unlefs I am greatly deceived; been hitherto rightly explained.

The firft of the characters preserved by the medal I am confidering is taken by Sig. Abate (8) Venuti for *Koph*, and I intirely agree with him in that notion. On my piece, however, it muft be allowed fomewhat different both from the correspondent element given us by this celebrated antiquary and that which occurs on the similar medals published by (9) M. Pellerin. It feems to refemble, though not very strongly, the letter *Kappa*, as it appears on certain antient Greek coins. I am nevertheless fully fatisfied, from feveral confiderations, that the cha-

(3) Paruta *Medagl. &c.*

(4) Laftanofa *Muf. de las Medallas Defconocid. &c.* En Huesca, 1645.

(5) Maffe. *Veron. Illuftrat. Lib. III. c. vii. p. 259, &c.*

(6) Ridolfin. Venut. ubi fup.

(7) Peller. *Recueil de Medailles de Peuples & de Villas, &c.* Tom. III. p. 85, 86. A Paris, 1763.

(8) Ridolfin. Venut. ubi fup.

(9) Peller. ubi fup.

rafter in question must be a figure of *Koph*. The legend on the similar medals published by (10) Sig. Abate Venuti and M. Pellerin renders this indisputably clear. That character therefore will, I doubt not, be looked upon by those well versed in this branch of literature as a new form of the element *Koph*.

The second and third letters of our inscription, as they appear on the coins communicated to the learned world by Sig. Abate Venuti and M. Pellerin, are apparently the same, though on mine they are most certainly distinct characters; the first of them strongly resembling a form of the Punic or Phœnician *Lamed*, and the other being indubitably one of *Nun*. Admit this, and the word may be read KAVLIN, or CAVLIN; though the *Yod*, after the Punic and Phœnician manner, is here suppressed. Such a suppression amongst the Phœnicians and Carthaginians was by no means uncommon, as I have (11) elsewhere incontestably proved. This being allowed, we shall, perhaps, not find it so difficult to point out the place where all these medals were struck.

There is a small island in the Mediterranean only five miles from Malta, denominated antiently ΓΑΥΛΟΣ, or GAVLOS, both by the Greeks and the Romans; as we learn (12) from Diodorus Siculus, (13) Mela, and (14) Pliny. This island, which is

(10) Ridolfin. Venut. & Peller. ubi sup.

(11) *Philosoph. Transact.* Vol. LIII. p. 275.

(12) Diod. Sic. Lib. V.

(13) Pompon. Mel. *De Sit. Crb.* Lib. II. c. vii.

(14) Plin. *Nat. Hist.* Lib. III. c. viii. Lib. V. c. vii.

about thirty miles in circumference, was (15) occupied by the Phœnicians in very early times, and afterwards by the (16) Greeks. When the latter were possessed of it, the capital, named also GAVLOS, was one of those cities called by the Greeks ΑΥΤΟΝΟΜΟΙ, governed by it's own laws, and consequently, as it should seem, a kind of free independent state. This may be fairly inferred from several antient coins of that city, to be met with in the cabinets of the great and the curious, with the Greek word ΓΑΥΛΙΤΩΝ upon them. The Carthaginians therefore (17), who probably succeeded the Greeks in the occupation of this island, may reasonably be presumed likewise to have coined money in that capital, with a Punic inscription upon it. Nor can this well be denied, as medals of Cosyra, or Cossura, denominated by the moderns Pantallaria, adorned with such an inscription (18), sometimes occur; though that island, whatever figure it might have made when possessed by the Carthaginians, was considerably smaller than GAVLOS, known at present by the name of Gozo. The Phœnicians and Carthaginians seem to have assigned the latter, when masters of it, the appellation of $\kappa\alpha\upsilon\lambda$, KAVL, or CAVL; which may, perhaps, be deemed equivalent to the (19) Hebrew $\kappa\alpha\lambda$, KAL, LIGHT,

(15) Diod. Sic. ubi sup. Phil. Cluver. Sicil. Antiqu. Lib. II. p. 444. Lugd. Batavor. 1619.

(16) Sil. Italic. Lib. XIV. ver. 274. Phil. Cluver. ubi sup. p. 445.

(17) Scyl. *Peripl.* Burchard. Nidersted. *Malt. Vet. & Nov.* p. 35. Helmeftadii, 1660.

(18) *Saggi di Dissertazion. Accademich. &c.* Tom. I. p. 31.

(19) Val. Schind. *Lex. Pentaglot.* p. 1615. Hanovix, 1612.

SMALL, or the (20) Arabic أَكْصَل, AKAL, the comparative of أَصْغَر, KAL, SMALLER; those nations considering Gozo as a smaller island, state, or district, than Malta. As for the minute island of Hephæstia, or Lopadusa (21), though mentioned by several of the antients, it was looked upon as meriting little attention, or regard. The etymon laid down here seems more apposite and natural than that obtruded upon the learned world by Bochart.

If the Phœnicians and Carthaginians then called Gozo, when occupied by them, CAVL, an inhabitant of that island must have been named by them CAVLI; which in the plural will give CAVLIM, or CAVLIN, according to the different ages in which the monuments exhibiting the word at first appeared. That IN was sometimes a Punic plural termination, the BAALSAMEN of St. Austin (23) seems clearly enough to imply; the word SAMEN being of the Chaldee dual form, and evincing the plural as well as the dual number in Punic masculine nouns to have ended sometimes in IN. Several of the antient Maltese, or Punic, numerals, published by Girolamo

(20) Jacob. Gol. Lexic. Arabico-Latin. p. 1950, 1951. Lugduni Batavorum, 1653.

(21) Plin. *Nat. Hist.* Lib. V. c. vi. Ptol. *Geograph.* Lib. IV. c. iii. Athen. *Deipnosoph.* apud Bochart. *Chan* p. 554, 555. Father Froelich mentions a coin of this minute island, with the word ΛΟΠΑΔΟΥΣΣΑΙΩΝ upon it; from whence we may conclude, that the true antient name of it was ΛΟΠΑΔΟΥΣΣΑ, ΛΟΠΑΔΥΣΣΑ, the authority of medals being deemed incontestable by the learned. Erasmi. Froel. *Notit. Elementar. Numism. Antiquor.* &c. p. 97.

(22) Bochart. *Chan.* Lib. I. c. xxvi. p. 554. Francofurti ad Moenum, 1681.

(23) August. *Locution.* Lib. VII. c. i.

(24) Me-

(24) Megiferio and (25) Johannes Henricus Maius, not to mention the Punic (26) ABELONII of St. Austin, and other similar instances, that might, with equal facility, be produced, manifestly prove the same thing. Nor ought it to appear strange, that the Punic inscription here should be expressed in the nominative case; as a parallel instance occurs on the reverse of a * (26) curious Punic coin of the island of Cossura, now in the very valuable cabinet of the Reverend and learned Mr. Godwyn, Fellow of Balliol College, Oxford, which is perfectly well preserved. As *Koph* therefore in (27) oriental names was not infrequently converted into *Gamma*, or *G*, both by the Latins and the Greeks, when such names were adopted by those nations; it can be no matter of surprize, that *CAVL*, by the aforesaid conversion, and the addition of a Greek or Latin termination, should become *GAVLOS*, the appellation assigned the isle of Gozo by several antient writers. Hence we may conclude it more than probable, that

(24) Girolam. Megiser. in *Descript. Melit.* c. i. f. 13. Lipsiæ, 1606.

(25) Johan. Henric. Maius in *Specim. Ling. Punic. in hodiern. Melitenf. superst.* § 16. f. 482. apud Petr. Burman. in *Thef. Antiq. &c.* T. XV.

(26) August. de *Hæresib. &c.* c. lxxxvii. Boch. *Chan. Lib. II.* c. xvi. p. 851. Francofurti ad Moen. 1681.

* (26) As some of the forms of the letters on this valuable coin differ from those of the correspondent elements in all the draughts of the similar pieces that I have hitherto met with, it seems to me highly to merit the attention of the curious and the learned. See *TAB. XI.* n. 3.

(27) Bochart. ubi sup. *Lib. I.* c. xlii. p. 737. c. xxvii. p. 569. & in *Phal. Lib. I.* c. iii. p. 20. Francof. ad Moen. 1681.

the piece in question was struck in Gozo, though the precise time of that operation cannot now be so easily ascertained.

From what has been said, I flatter myself, the learned will admit the medal before me to have appeared first in the isle of Gozo, and not in Malta, as some of the most celebrated antiquaries have supposed; especially, since the people of the former island had a mint erected in their capital, from whence coins were emitted, with the word ΓΑΥ-ΑΙΤΩΝ on the reverse (28), when under the domination of the Greeks. One of those coins has a place assigned it in my small cabinet, and another in the valuable collection of the Reverend and learned Mr. Cracherode, Student of Christ-Church, Oxon. My piece in the main is well preserved, though the three last letters of the legend on the reverse have been effaced by the injuries of time; but Mr. Cracherode's, which is likewise in pretty good conservation, has handed down to us that legend perfect and intire. A draught of my medal attends this paper (see TAB. XI. n. 2.), which for accuracy may be absolutely depended upon. We may safely therefore, as I apprehend, attribute the Punic medal I am considering, as well as all others adorned with the same Punic characters, to the isle, or city, of Gozo, to which it seems in reality to belong.

To what has been here advanced it may possibly be objected by some, that the island of Malta is much superior, both in number of inhabitants and extent, to that of Gozo; and that most of the antient coins

(28) Peller. ubi sup. p. 34, 35, 36.

similar to that before me are brought from Malta, where they are not infrequently found. From whence they may be inclined to collect, as Sig. Abate Venuti (29) actually has done, that all such Punic medals as that I am attempting to explain made their first appearance in the island of Malta. In answer to the first of these objections, I would beg leave to reply, that as the two islands here mentioned were so near one another, both of them free states, coined their own money, were occupied by people of the same nation, and consequently of the same religion; neither their extent nor the number of inhabitants they respectively contained can be of any great weight, with regard to the point in question. But farther, the island of Gaulos, or Gozo, though lesser than Malta, was so considerable, probably by reason of it's excellent ports, mentioned by (30) Diodorus Siculus, that Mela (31) and Pliny (32), in their short list of the islands of the Sicilian sea, towards the coast of Africa, place it even before Malta, the largest of them. And we learn from an antient inscription, that it had the honour of being a municipium (33) in the Roman times. Nor will the second objection be of any great force, when it is considered, that the two islands of Malta and Gozo were in (34) a

(29) Ridolfin. Venut. ubi sup. p. 37.

(30) Diod. Sic. ubi sup. Lib. V.

(31) Pompon. Mel. ubi sup.

(32) Plin. ubi sup. p. 164.

(33) Spon. *Misc. Erudit. Antiq.* Christ. Cellar. *Notit. Orb. Antiq.* p. 655, 656. Cantabrigiæ, 1703.

(34) Job. Quintin. in *Descript. Melit.* & Burch. *Nidersted.* ubi sup. p. 35, 36.

manner contiguous to one another; that the money of each was undoubtedly current, on account of their great vicinity, in both of them; and therefore that we may easily conceive a considerable number of pieces appertaining antiently to the latter to have been preserved, for many ages, in the former of those islands. And this is rendered still more probable by one or two inscriptions found in Malta (35), that originally belonged to the isle of Gozo. Nor is the cardinal point taken for granted in this objection, on which it almost intirely turns, by any means incontestable. For several of these medals, perhaps most of them, are brought into Europe from Tunis, as I take mine actually to have been, to which place they might as easily have found their way from Gozo as from Malta. All which being maturely weighed, the figures of the letters themselves, which ought to be the basis of all our reasoning relative to the word formed of those letters, will, I would flatter myself, be allowed decisive in favour of my present opinion.

Sig. Abate (36) Venuti has, indeed, observed, that the God Mithra sometimes appears on such Punic coins as that considered by me here, in the same manner as on some of the antient Greek (37) medals of the Maltese; from whence he infers, that these Punic pieces are to be attributed to the island of

(35) Spon. *Misc. Antiq.* p. 190, 192. Jan. Gruter. *Corp. Inscript. ex Recens. & cum Annotat.* Joan. Geor. Grævii, p. 415. Amstelædami, 1707. Christoph. Cellar. *Notit. Orb. Antiq.* p. 655, 656. Cantabrigiæ, 1703.

(36) Ridolfin. Venut. ubi sup. p. 37.

(37) *Mem. de l'Acad. des Bell. Lett.* Tom. IX. p. 160.

Malta. But this inference, as I apprehend, is neither valid nor just. For as the inhabitants of Gozo were of the same religion with those of Malta, as has been already remarked; 'tis natural to suppose, that the former, as well as the latter, might have impressed the effigies of the God Mithra, and any other religious, or rather superstitious, symbol, common to them both, on their coins.

I shall only beg leave at this time to add, that the MSS. from which some of the earlier editions (38) of Silius Italicus were printed, exhibited *CAVLVM*, as the true antient name of the isle of Gozo. Phil. (39) Cluverius, Christoph. (40) Cellarius, and Nic. (41) Heinsius, I know, look upon this lection as a corruption, and scruple not to pronounce it a depravation of the text. But their bare assertion, intirely unsupported, as it is, and strongly opposed by a Punic medal of undoubted antiquity, by no means convinces me of the truth of what they assert; especially, as there are other lections of the antient name of Gozo, two of which have for their initial letter K, or C. This

(38) Sil. Ital. Lib. XIV. ver. 274. Edit. Colin. Parisiis, 1531. Sil. Ital. ubi sup. cum Argumentis Hermannii Buschii. Lugduni, 1598. Id. ibid. Ed. Plantin. 1600. Id. ibid. Lugduni, 1603. Id. ibid. Ed. Crispin. in Corp. Vet. Poet. Latin. 1601. Id. ibid. cum Comment. Dausqueii Sanctomarii. Parisiis, 1618.

(39) Phil. Cluver. *Sicil. Antiq.* Lib. II. p. 444. Lugd. Batavor. 1619.

(40) Christoph. Cellar. in Sil. Ital. Lib. XIV. ver. 274. Lipsiæ, 1695.

(41) Nic. Heinss. in Sil. Ital. ubi sup. Ed. Drakenb. Trajecti ad Rhenum, 1717.

we learn from (42) some of the manuscripts, and the printed copies, of Strabo. The authority of these manuscripts, in conjunction with that of those of Silius Italicus abovementioned, seems to bring a fresh accession of strength to the preceding interpretation of the Punic word on my coin, and at the same time to receive itself no inconsiderable support from that interpretation.

You will excuse the trouble given on this occasion, as some light may possibly be thrown on several very curious and valuable Punic medals, hitherto unexplained, by the paper now sent you; and believe me to be, with all possible consideration and regard,

Good Sir,

Your most obedient humble servant,

Christ Church, Oxon.
Sept. 2, 1768.

John Swinton.

(42) If. Casaub. *Comment. et Castigat. ad Lib. Strabon. Geograph.* I. VI. VII. Gio. Pietro Francesco Agius de Soldanis, *Dell' Origine Della Lingua Punica presentamente usata da Maltesi, Dissertaz.* I. p. 50. In Roma, 1750.

P. 235, l. 13, 14, for n. 5. insert n. 1. P. 249, l. 9. for n. 2, 3. insert n. 1, 2.
P. 253, l. 13, for n. 1. insert n. 3.

Received September 29, 1768.

XXXVII. *Elucidation of an Etruscan Coin of Pæstum, in Lucania, emitted from the Mint there, about the Time of the Social War. In a Letter to Charles Morton, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Dear Sir,

Read Nov. 24, 1768. **T**HE coin adorned with Etruscan characters, of which you will meet with a description (see TAB. XI. n. 4.) in this letter, at present in my small cabinet, was some years since communicated to the learned world by Sig. Passeri; and ascribed to the city (1) of Pæstum, in Lucania, of which such noble ruins are still extant, by that ingenious author. This notion, upon farther examination, will be found by no means remote from truth; though, by attributing a wrong power, as I apprehend, to one

(1) Joan. Baptist. Passer. *De Num. Etrusc. Pæstanor. Dissertat.* in *Symbol. Litterar. &c.* Vol. secund. p. 13—35. Florentiæ, 1748.

of the characters of the legend or inscription on the reverse, and consequently assigning a false lesson to that inscription (2), Sig. Passeri seems greatly to have shaken, at least, if not intirely overturned, his own opinion.

The head, with curled hair, on one side of this curious minute coin, may not improbably (3), as Sig. Passeri believes, be the effigies of some famous hero, or general, if not the founder of a city, that antiently bore a relation to the place where the piece was struck ; or it may possibly, (4) as the same learned gentleman also suggests, be allowed to point out to us some local deity. Two of the symbols on the reverse undoubtedly represent a dolphin and an acrostolium, though what that between these two was intended to point out to us, I cannot, with the same facility, take upon me to decide. The globule in the middle of this last, as it is termed by Sig. (5) Passeri, is most evidently on my medal such a concha marina, or sea shell, as we sometimes meet with on antient coins. This perfectly well agrees with the two symbols abovementioned, and, in conjunction with them, clearly evinces the piece in question to have been the produce of a mint erected in a maritime town.

It may not be improper to observe here, that a silver Greek medal, in fine conservation, with the word KYMAION on the reverse, in an oriental direction, from the right hand to the left, and two

(2) Id. *ibid.* p. 17.

(3) Id. *ibid.* p. 21.

(4) Joan. Bapt. Passer. *ubi sup.*

(5) Id. *ibid.* p. 27, 28.

figures extremely similar to those under the dolphin on my coin, which he takes to belong to the city of Cuma in Campania, has been published by (6) M. Pellerin.

With regard to the legend or inscription, preserved on the reverse, it may not be improper to remark, that the fifth letter, taken (7) by Sig. Passeri for L, is, at least in my opinion, most certainly v. This is sufficiently apparent from even the four coins published by that ingenious gentleman, in the piece (8) here referred to; and still more so from the medal now before me, as well as from another in the valuable collection of the Reverend and learned Mr. Cracherode, Student of Christ-Church, both of which are in the finest conservation, and clearly exhibit vv, not LV. In other respects, the lection assigned this minute inscription (9) by Sig. Passeri seems nearly to approach the truth, if it be not perfectly true. I shall therefore take the liberty to read it ZIVVTZIS, PHISTVVIS; which, according to the cacography, or uncouth manner of writing, of the Etruscans (10), mentioned by me in a former paper, may, with sufficient propriety, be deemed equivalent to the Latin PÆSTVM. The inscription will, however, become quite another word, by the conversion of the first v into L, essentially different from the Latin name of the city wherein Sig. Passeri supposes it to

(6) Peller. *Recueil de Medailles de Peuples & de Villes, &c.* Tom. I. p. 47. pl. viii. n. 23. A Paris, 1763.

(7) Jo. Bapt. Passeri. ubi sup. p. 19.

(8) Id. ibid. p. 15—17.

(9) Id. ibid. p. 19, 20.

(10) *Philosoph. Transact.* Vol. LI. Par. II. p. 858.

have been struck; the letter L by no means, as a radical, entering into the composition of that name. Nor is the last element of the word (11) MVTIL, in any of the (12) most accurate draughts of Sig. Olivieri's Samnite-Etruscan coins of Papius Mutilus, found to resemble v, as Sig. Passeri has been pleased to assert. That element in all those draughts, as well as on the reverses of two medals of the same general in my small collection (see TAB. XII. n. 2, 3.), perfectly well preserved, is most apparently L, in the true Etruscan, or rather Samnite-Etruscan, form.

If this notion, which to me appears incontestable, should be admitted by the learned, they will of course reject the wild and arbitrary supposition of a modern Italian writer (13); who attributes the piece I am considering to Plisfia, an obscure inconsiderable town, mentioned (14) by Livy, and scarce ever, as far as I can recollect, by any other antient author. For the expunction of the L, a letter here purely ima-

(11) Sig. Gori has remarked, that one of the sides of the Etruscan v is now and then somewhat longer than the other. When this happens, part of the shorter side may be supposed to have been erased by the injuries of time; which, indeed, from the instances he has adduced, seems highly probable. Be that, however, as it will, even such an incomplete v as this is very distinguishable from the Etruscan L; as the principal part, or longer line, of the latter either always is or ought to be a perpendicular: whereas both the sides of the former are in an oblique position, though not always equally so. Anton. Francisc. Gor. *Mus. Etrusc.* p. 414. Florentiæ, 1737.

(12) *Saggi di Dissertaz. Accad. pub. lett. nella Nob. Accad. dell' antichiss. Cit. di Corton.* Tom. IV. p. 133. In Roma, 1743.

(13) Paschal. Magnon. *De Ver. Posidoniae et Pæstii Originibus, &c.*

(14) Liv. Lib. IX. c. xiii. xiv.

VOL. LVIII. K k

ginary,

ginary, and the substitution of the v, in its stead, which the medal itself fully justifies, will not, as I apprehend, leave the least room for so precarious, not to say absurd, a supposition. Nor will many of the learned think this too harsh an appellation, as Plistia (15) seems to have been an obscure inland town; whereas the symbols on the reverse of my coin plainly evince it to have been struck in a maritime city, and a city of very considerable note. It would therefore have been a little unlucky for a certain English writer (16) to determine in favour of the Italian author abovementioned, and assign this piece, as well as all others similar to it, to Plistia, as he has done, had he been fully acquainted with the subject he wrote upon; but as this seems not to have been the case, his character, as an adept in this branch of literature, will not be greatly affected by the mistake.

With regard to the antiquity of the medal I am now upon, I shall beg leave to remark, that the forms of the letters it exhibits, so perfectly similar to those of the elements preserved by the coins of Papius Mutilus and Tiberius Veturius, heretofore explained, clearly indicate it to have been struck about the time of the Social war. Besides, that war is known to have raged in (17) Lucania; and the Lucanians are said to (18) to have been one of the

(15) Phil. Cluver. *Ital. Antiq.* Lib. II. c. xv. p. 772. Lugd. Batavorum, 1634.

(16) See *The Ruins of Paestum*, p. 37. Lond. 1768.

(17) Appian. Alexandrin. *De Bell. Civil.* p. 375. See also *Philosoph. Transact.* Vol. LII. Part 1. p. 29, 30.

(18) Aut. Epit. Livian. LXXII. LXXIII.

principal nations concerned at that time in the revolt from the Romans, if they did not actually take the lead in that revolt. They were commanded by M. Lamponius and Tiberius Cleptius, two able generals; the former of which defeated a body of Roman troops, under the command of Licinius Crassus, formed the siege of Grumentum, in Lucania, and not a (19) little distinguished himself, either in the first or second campaign of the Social war. The Etruscan, or rather Samnite-Etruscan, characters on this Lucanian piece are therefore a very good proof of the truth of what is here advanced. For the principal confederated Italian states, during the course of that war, used the Etruscan characters, the antient letters of Italy, and that on their coins, out of despite to the (20) Romans. This has been already observed by the very ingenious Sig. Olivieri, in the piece here referred to; nor will it, I would persuade myself, be contested in any part of the learned world.

The celebrated Sig. Passeri supposes L to have (21) entered into the composition of the Etruscan name of Pæstum, by which he in a great measure, if not intirely, overturns his own hypothesis, relative to the place where the coin before me first appeared. In order to account for this strange supposition, he asserts the antients frequently to have placed the letter L after s; and (22) produces the words STLATVM,

(19) Appian. Alexandrin. ubi sup.

(20) *Saggi di Dissertazioni Accademiche pubblicamente lette nella Nobile Accademia Etrusca dell' antichissima Città di Cortona.* Tom.

II. p. 53. In Roma, 1738.

(21) Jo. Bapt. Passer. ubi sup. p. 19, 20, 21.

(22) Id. ibid. p. 20, 21.

STLITES, for LATVM, LITES, in support of his assertion. But here ST, not simply s, is added to L, the first radical letter, as an obsolete adjection, by no means essential to the word; whereas, in his Etruscan name of Pæstum (23), ST, not s, precedes L, by no means as an extrinsic addition, but as appertaining to the radix, and consequently as an essential part of the name. This therefore by no means comes up to the point. The proof is altogether as preposterous as the thing to be proved. But as I have already exceeded the limits I at first proposed to myself in this paper, it is time to conclude; which you will permit me to do, with assuring you that I am,

Dear Sir,

Your very faithful,

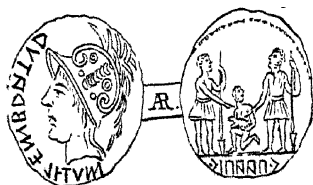
and most obedient humble servant,

Christ-Church, Oxon.
Sept. 5, 1768.

John Swinton.

(23) Id. *ibid.* p. 19.

n. 1.



*Penes Joannem Swinton, S.T.B.
Oxonienf. R.S.S.*

n. 2.



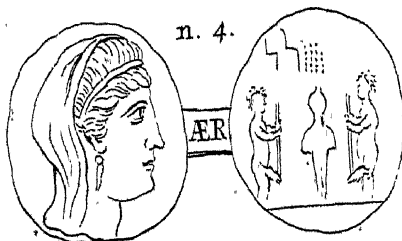
*Penes Joannem Swinton, S.T.B.
Oxonienf. R.S.S.*

n. 3.



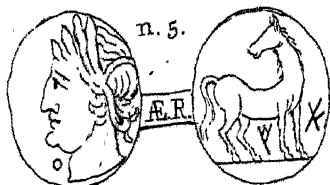
*Penes Joannem Swinton, S.T.B.
Oxonienf. R.S.S.*

n. 4.



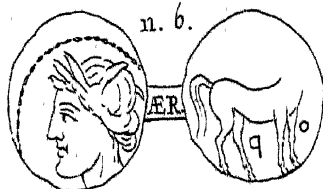
*Penes Joannem Swinton, S.T.B.
Oxonienf. R.S.S.*

n. 5.



*Apud Joannem Swinton, S.T.B.
Oxonienf. R.S.S.*

n. 6.



*Apud Joannem Swinton, S.T.B.
Oxonienf. R.S.S.*

Received November 1, 1768.

XXXVIII. *Remarks upon a Denarius of the Veturian Family, with an Etruscan Inscription on the Reverse, never before published. In a Letter to Mathew Maty, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read Dec. 22, 1768. **I** HAVE now in my small cabinet (see TAB. XII. n. 1.) a Samnite-Etruscan denarius, brought some years since by a gentleman of this University from Rome, resembling one of the Veturian family, by me formerly explained, in every particular but the legend or inscription, on the reverse, and the letter in the exergue. As it will be needless to repeat what has already been said, relative to this species of coins, I shall beg leave to refer, for a description of the medal before me, to one of (1) my

(1) *Philosoph. Transact.* Vol. LII. Par. I. p. 28—39. former

former papers; and confine myself at present to the consideration of the inscription on the reverse, which seems to merit the attention of the curious, and has not hitherto been communicated to the learned world. An attempt therefore to interpret this will not, I would flatter myself, prove unacceptable to the Royal Society; especially, as some light may possibly be thereby thrown on several other similar coins.

The inscription now in view consisted originally, as I apprehend, of either five or six letters. If M. be taken for the prenomén here, the number of them amounted only to five; but if NI. be supposed to precede the name on this piece, six undoubtedly at first appeared. I was for some time disposed to close with the former of these notions, as M. is a prenomén that sometimes occurs on the consular coins; as the three perpendiculars forming so considerable a part of the Samnite-Etruscan M are not always equidistant from one another, on such medals as that before me; and as one of the transverse lines of the Samnite-Etruscan character representing that element, on these coins, might have been easily effaced by the injuries of time. But examining the letters with greater attention, I afterwards found myself rather inclined to pronounce that part of the inscription terminated by a point NI. though such a prenomén has, perhaps, been scarce ever hitherto discovered on any ancient monument. However, I will not presume to determine absolutely in favour of either of these points, though one of them must certainly be true. The former receives some countenance from two coins of Papius Mutilus, in my little collection, which exhibit the middle perpendicular of the Samnite-

nite-Etruscan M as not equidistant from the other two; and the latter, in my opinion, appears at least equally probable from the face of the inscription. Accurate (2) draughts of both these medals of Papius Mutilus, one of which is in the finest conservation, may be seen in the table referred to here. Of the two foregoing notions that will be judged the most agreeable to truth, which seems the most eligible to the learned.

The prenomen being thus dispatched, I proceed to the name of the Italian commander preserved on this coin. The first letter is apparently the Etruscan L. The second is as evidently ∇ , adorned with the Samnite-Etruscan accent, which (3) gave it the power of the Greek diphthong OY , or ov . This here is only a point, equidistant from the two sides; which confirms what I formerly suggested, in relation to (4) that accent. It is very visible on the medal I am now endeavouring to throw some light upon. Part of the third letter has been defaced, but the remainder sufficiently indicates it to have been the Samnite-Etruscan P. Some faint traces of both sides of the fourth element appear, which will probably be allowed to announce it v. The Samnite-Etruscan accent in the former ∇ , which conferred upon it the power of OY , or ov , and distinguished it from the simple v, however, does not present itself to our view here. Thus stand the characters forming part of a name which, if I am not much mistaken, will soon be more fully explained.

(2) *Philosoph. Transact.* Vol. LVIII. Tab. XII. n. 2, 3. p. 255.

(3) *Philosoph. Transact.* Vol. LII. Par. I. p. 32, 33.

(4) *Philosoph. Transact.* Vol. LII. Par. I. p. 33.

As neither the Samnites nor the Etruscans had in their alphabet o, they used the simple unaccented v for that element. This is evident from the celebrated tables of Gubbio, from the coins of Papius Mutilus, and from other Etruscan and Samnite remains of antiquity. This we also learn from (5) Festus, (6) Quintilian, and other antient writers of good repute. I shall therefore not scruple to consider the last letter of the inscription I am now upon, supposing it v, as I verily believe it was, as equivalent to the Latin or Roman o, and consequently shall read the four elements at first impressed upon the coin LVPO. This I would also look upon as part of the name LVPONIVS, though I question whether or no that name has been ever yet observed on any other monument. Be that, however, as it will, the truth of my notion, from what immediately follows, will, I flatter myself, more clearly appear.

The Lucanian forces acted under the orders of M. Lamponius and Tiberius Cleptius, two generals of very considerable note, in the Social war. The former of these, according to (7) Appian, distinguished himself by the defeat of a body of Roman troops, under the command of Licinius Crassus, and the siege of Grumentum, in Lucania, either in the first, or second campaign of that war. As therefore, by such a blow, he must have rendered no small service to the common cause; 'tis natural to suppose, that the allies did him the honour of impressing his name on some of their coins. And that

(5) S. Pomp. Fest. p. 339. Lutetiae Parisiorum, 1681.

(6) M. Fab. Quintilian. Lib. I. c. 4. Mar. Ver. Flac. apud Fest. ubi sup.

(7) Appian. Alexandrin. *De Bel. Civil.* p. 375.

this was really the case, we may fairly presume, as Papius Mutilus (8) and Tiberius Veturius, two other Italian generals, his cotemporaries, were actually treated with the same mark of distinction, at the same time, for their laudable conduct in the Social war. This we learn (9) from several antient medals, adorned with the names of those commanders, that have been heretofore explained. I would therefore convert the M. LAMPONIVS of the printed copies of Appian into M. or NI. LVPONIVS (10), the prenomen and name pointed out to us by my coin. I say M. or NI. LVPONIVS, because it seems altogether immaterial whether we assume M. or NI. for the prenomen here; though the latter of these, I believe, has scarce ever hitherto occurred on any of either the Roman or Etruscan remains of antiquity. Notwithstanding which, I find myself inclined to prefer it to the other, as has been intimated above. And in farther support of this preference it may be remarked, that NI. in a manuscript might easily have been taken for M., so similar are they to each other (especially if the side of N next to I had been by any accident defaced), by a careless transcriber. The medal itself most certainly seems to determine in favour of this opinion, though what the complete prenomen represented by NI. really was, I must not at present take upon me absolutely to decide.

(8) *Philosoph. Transact.* Vol. LI. Par. II. p. 853—865. & Vol. LII. Par. I. p. 28—39.

(9) *Philosoph. Transact.* ubi sup.

(10) Perhaps the prenomen of this commander was Nigidius. Other generals of the confederated rebels were denominated Herius Afinius, Marius Egnatius, &c. which may possibly seem to give a sort of sanction to such an opinion. Aut. Epit. Livian. Vell. Paterc. Appian. &c.

From what has been said it appears highly probable, that M. or NI. LVPONIVS stood originally in the text of Appian, or rather in that of Cornelius Sisenna (11), who gave a very minute and particular description of the Social war, or of some other Latin author followed by the Greek historian. That writer therefore seems to be emended, in the instance before us, by my coin; which will perhaps be allowed to have brought to light the true name of an Italian general, who commanded a body of the confederated rebels in the Social war, that had been lost for many ages. And this is rendered still more probable by the Samnite-Etruscan accent, which makes the first \vee in that name equivalent to the Greek diphthong oy , or ov . For that diphthong and L form the first syllable of LVPVS, or ΛΟΥΠΙΟΣ , from whence the word LVPONIVS seems apparently to have been derived, as we learn from the text of (12) Appian. Nor can it be denied, that (13) LVPIVS and LVPONIVS are, by analogy, as naturally deducible from the Roman or Italian cognomen LVPVS as APRIVS and APRONIVS (14) are from APER: whereas LAM-

(11) Corn. Sifen. *Hist. Lib. IV.* apud Non. Marcel. in voc. CONVENIRE, cap. iv. col. 633. l. 48, 49, 50. Geneva, 1622.

(12) Appian. Alexandrin. *De Bel. Civil. Lib. I.* p. 374. Amstelodami, 1670.

(13) Lud. Ant. Murator. *Nov. Thesaur. Inscript. &c.* p. 1264. Vid. etiam *Ind. Univers. Class.* 17 p. 2302. Mediolani, 1742.

(14) The family name APRINIVS, which sometimes occurs, seems to be altogether the same with APRONIVS; the position of I in the place of o , and o in the place of I , having not been uncommon amongst the antients. Of this QVICVM for QVOCVM, DIE CRASTINI for DIE CRASTINO, AGNOTVS for AGNITVS, OLLI for ILLI; to omit many other instances that
PONIVS,

TONIVS, the name exhibited by the printed copies of the last mentioned author, seems to carry along with it the air of a depravation; since no Greek or Latin source, from whence we can, with sufficient propriety, suppose it to flow, presents itself to our view.

From the preceding observations, which, I flatter myself, will be deemed by no means remote from truth, it will follow, that the medal before me is to be attributed to the Veturian and Luponian families. But instances of this kind (15) pretty frequently occur. The names of Tiberius Veturius and C. Papius Mutilus are (16) exhibited by another Samnite-Etruscan denarius, of the same age with this, which I have formerly explained; and several Roman consular coins struck about the time of the Social war, and the following age, adorned with the names of two illustrious (17) persons of different families, are to be met with in the cabinets of the great and the curious at this very day.

If what has been here advanced should meet with the approbation of the learned, they will not read

might be adduced, are incontestable proofs. In like manner LVPINIVS and LVPONIVS might, by analogy, have been deemed antiently the same name; the second syllable of the latter of which the Samnites and Etruscans, for want of o, undoubtedly wrote pv. Lud. Ant. Murat. *Nov. Thesaur. Vet. Inscript.* Tom. III. p. mccc. n. 12. Mediolani, 1740. Joh. Nic. Func. *De Adolescent. Lat. Ling. Tract.* p. 324. Marburgi Catalogorum, 1723.

(15) Vid. Vaill. Patin. Morell. & Sig. Haverc. in *Num. Fam. Rom.*

(16) *Philosop. Transact.* Vol. LII. Par. I. p. 21—39.

(17) *Philosop. Transact.* ubi sup. p. 31.

the short inscription I have been considering *NIGRIDIVS LVPVS*, which I was once inclined to do; as a person of that name is not, I believe, to be met with in the whole circle of antiquity. Such a lection as this would not have the sanction of any antient writer, of any antient coin, or of any other antient monument; whereas my lection, or rather my interpretation of what I conceive to be the true lection, of this name, is supported by the authority of Appian, which in it's turn likewise receives no small accession of strength from this interpretation.

I shall only beg leave to add, that this interpretation, if admitted by the learned, will also enable us to add the Luponian family, as a new one, to those collected by Dr. Vaillant and M. Morell, whose names, together with some of the actions of their most illustrious members, have been handed down to us, through a long series of ages, by antient coins; and that I am, with great truth,

S I R,

Your most obedient humble servant,

Christ-Church, Oxon.
Octob. 31, 1768.

John Swinton.

Received Nov. 14, 1768.

XXXIX. *Description of a Punic Coin appertaining to the Isle of Gozo, hitherto attributed to that of Malta, by the Learned. In a Letter to Mathew Maty, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read Dec 22, 1768. **T**HE Punic medal before me (see TAB. XII. n. 4.), of which I send you a short account in this paper, has been published (1) by F. Montfaucon, (2) the Marquis Scipio Maffei, and Sig. (3) Abate Venuti; but not by Paruta and Lasta-

(1) Montfauc. *Ant. Expl.* T. II. par. 2. p. 293.

(2) Maff. *Veron. Illust.* Lib. III. c. vii. p. 259. In Verona, 1732.

(3) Ridolfino Venuti. *Dissertaz. sopra alcun. Medagl. Maltes.* in *Saggi di Dissertaz. Accademich. pubblic. let. nella Nobil. Accadem. Etrusc. dell' antichis. Cit. di Cortona.* Tom. I. p. 36, 37. In Roma, 1735.

noſa,

noſa, as Sig. (4) Abate Venuti has been pleaſed to aſſert. On one ſide the head of a woman veiled preſents itſelf to our view, and on the other three Egyptian figures, according to the Marquis Scipio Maffei. 'Tis obſervable that my medal, as well as that communicated to the learned world by the laſt mentioned author, exhibits a ſort of wings fixed on the hips of the two exterior figures, though nothing like ſuch wings is viſible on the ſimilar medal publiſhed by Sig. Abate Venuti. M. l'Abbé Barthelemy (5) may be ſuppoſed to have had an eye to this coin, when he informed us, “ that the god Oſiris appears with his attributes on the medals which the Phœnicians ſtruck “ in the iſle of Malta;” and to have conſidered the ſymbols on the reverſe, whatever they were originally expreſſive of, as relative to the worſhip of Oſiris which prevailed amongſt the Phœnicians in that iſland. The Marquis Scipio Maffei ſeems to take the whole type to be Egyptian, and to point out to us ſome mode of the Egyptian ſuperſtition; but Sig. Abate Venuti will have the figure in the middle to be the god (6) Mithra, and the other two worſhipers of that deity, each of them ſeeming to offer a patera to him. Which of theſe opinions is true, or whether any of them be ſo, I ſhall not at preſent take upon me to decide.

That this medal was at firſt adorned with a ſhort Punic inſcription on the reverſe, formed of the letters *Koph*, *Lamed*, and *Nun*, and conſequently ſtruck

(4) Venut. ubi ſup. p. 36.

(5) *Mem. de Litter. &c.* Tom. XXXII. p. 737. A. Paris, 1768.

(6) Venut. ubi ſup. p. 37.

in the isle of GAVLOS, or Gozo, is plainly deducible from the (7) draughts of it published by the Marquis Scipio Maffei and Sig. Abate Venuti. The latter, however, of those draughts approaches nearer the original than the former, with regard to the inscription, though neither of them gives us a perfect representation of the letters the piece exhibits. Of those letters the last only, or *Nun*, has been preserved intire on my coin, and this is so faint that it is little more than barely visible. Part of the second is just perceptible, and seems to indicate the whole to have been *Lamed*, as that element appears on the coin of Gozo by me formerly described. The first letter is so totally defaced that not the faintest traces of it can be discerned. I must not forget to observe, that the form of the *Nun* here is perfectly similar to, or rather exactly the same with, that on the medal of Gozo I have lately explained, though somewhat different from the characters endued with the power of that element on all the draughts of the coins of Gozo that have hitherto appeared.

It may not be improper to remark, that the piece I am considering will bring a fresh accession of strength to what has been advanced in (8) one of my former papers, relative to this species of coins, as well as to the Punic or Phœnician name of the people antiently inhabiting the isle of Gozo. I shall forbear at present drawing any other conclusion from the medal before me, or rather from the very faint remains of the Punic characters it has handed down to us,

(7) Scip. Maff. & Ridolfin.-Venut. ubi sup.

(8) *Philosop. Transact.* Vol. LVIII. TAB. XI. p. 235—245.
which

which are the principal object of my attention here. What has been farther offered by F. Montfaucon, the Marquis Scipio Maffei, and Sig. Abate Venuti, both with respect to the veiled head (9) and the symbols on the reverse, will probably be deemed little better than vague conjectures, scarce meriting the attention of the learned.

You will consider this as a small appendix to the paper lately sent the Royal Society on (10) a Punic coin, which I attributed to the isle of Gozo; and believe me to be, with the highest regard,

Good Sir,

Your much obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.

Nov. 10, 1768.

John Swinton.

(9) Montfauc. Maff. & Venut. ubi sup.

(10) *Philos. Transact.* Vol. LVIII. TAB. XI. p. 235—245.

Received November 14, 1768.

XL. *Observations on an inedited Coin, adorned with two Punic Characters on the Reverse. In a Letter to Mathew Maty, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read Dec. 24,
1768. **A**BOUT three months since, a friend of mine (see TAB. XII. n. 5.) brought me a small brass medal, exhibiting on one side the head of a woman, decked with ears of corn; and on the other a horse standing still, and looking behind him, or towards his tail, one of the usual symbols on the reverses of such Punic coins. The medal is pretty well preserved, and adorned with two Punic characters; one of which is placed near the horse's breast, and the other under his belly. Neither of them seems to have suffered much, if at all, from the injuries of time.

These two Punic letters may, as I apprehend, be safely pronounced *Aleph* and *Koph*, and must be considered as forming the first part of the name of

some noted city, either in Sicily or Africa. The learned, at least that part of them the most conversant in the branch of literature I am now upon, have frequently, if not generally, attributed such pieces as that before me to the island of Sicily. But M. l'Abbé Barthelemy, who differs from all other antiquaries in many of his notions, seems to reprehend me for adopting such a supposition; though he has himself ascribed several Punic coins, embellished with similar characters, to Sicilian towns. But it matters not where such pieces as this, with Punic characters upon them, first appeared, provided they were struck in places either dependent upon or in alliance with the Carthaginians. And that they were struck in places either dependent upon or in alliance with the Carthaginians, M. l'Abbé will not, I presume, deny; if he should, the symbols themselves, in conjunction with the characters preserved on these coins, would render this point sufficiently clear.

With regard to the coin I am now considering, as I cannot meet with any antient noted city of Africa, that had a mint erected in it, and a name beginning with the letters this piece exhibits; I cannot prevail upon myself, at least for the present, to attribute it to any town in that part of the world. I should rather think it might have belonged to Agrigentum, a very celebrated antient city of Sicily, where money was coined, when that part of the island in which it was seated either appertained to the Carthaginians, was in alliance with that people, or had some commercial connections with them. The most antient part of Agrigentum was denominated

minated AKPA, or ACRA, as we learn from the authors cited by (1) Bochart; and that the same name was used by the Carthaginians, it is by no means unreasonable to suppose. Perhaps the later Greek name AKPAΓΑΣ, or some other appellation similar to it, might also have been in vogue amongst them. In either of which cases, the Punic elements *Aleph*, *Koph*, would very well answer to the Greek letters *Alpha*, *Kappa*; as the latter of those letters is well known not seldom to have been equivalent to the (2) Phœnician, and consequently the Punic, *Koph*.

This seems still farther to appear from the draught of a medal of Agrigentum, published by (3) Paruta, with those two characters, and those two only, upon it. As the *Alpha* and *Kappa* there may, with great reason, be deemed equipollent to the two Punic elements on the coin here described; such an equipollence, or rather coincidence, of characters will be looked upon as an additional proof of the truth, or, at least, the probability, of the notion I would now recommend to the consideration of the learned world.

It must be here remarked, that under the chin of the female head a globule presents itself to our view; which may be considered as an uncial mark, denoting the weight or value of the piece. Such globules as this (4) not infrequently occur on the

(1) Bochart. *Chan.* Lib. I. c. xxix. p. 610, 611. Francofurti ad Moenum, 1681.

(2) *Philosopb. Transact.* Vol. LIV. p. *138, *139.

(3) Fil. Parut. *La Sicil. Num.* in *Num. di Gergent.* p. 9.

(4) Honor. Arigon. *Numism. quæd. cujuscunq; form. & metal.* &c. in *Antiq. Urb. & Populor. Numism. cum not. numeral.*

Etruscan, Greek, and Roman coins; but I remember not to have met with any author who has observed, that they appear sometimes on the Punic. I have another Carthaginian medal (see TAB. XII. n. 6.), with such a globule on the reverse; from whence we may conclude, that this uncial mark was used, on certain occasions, on both sides of the Punic coins. That the globule exhibited by the piece before me may be supposed an uncial mark, seems apparent, not only from the size of the medal itself, but likewise from the difficulty of accounting for it on any other supposition. If what is here advanced should meet with the approbation of the learned, it will be a farther proof that the coin was struck in Sicily; in which island, (5) and it's neighbourhood, many such antient pieces first appeared. That this, however, was really the case, I must by no means take upon me positively to affirm.

Tarvisin, 1741. Fil. Parut. ubi sup. pass. Anton. Francisc. Gor. *Mus. Etrusc.* p. 419—431. Tab. CXCVI. CXCVII. aliique scriptor. plur.

(5) *Idem* *ibid.* Many antient pieces, struck in Sicily, Magna Græcia, Etruria, &c. are adorned either with one or more of these globules; which are, with great reason, taken for uncial marks by the learned. The coins of Agrigentum, in particular, frequently exhibit such marks. One of the medals of that city has been published, by Paruta, with a single globule upon it, extremely similar to that handed down to us by the Punic medal considered here. Some of the globules on the Sicilian pieces, published by Paruta, are exceeding small; and, in this respect, greatly resemble that preserved by the Punic coin I have been endeavouring to explain. Fil. Parut. *De Sicill. Num.* in *Num. di Gergon di Drage Biume & alib.*

But

But to whatever town or country my medal originally appertained, it undoubtedly evinces the character I formerly took for *Koph* not to be *Aleph*, as some have supposed it to be, but in reality to be endowed with the power of *Koph*; though it has been ranked amongst the (6) forms of *Aleph*, in my Siculo-Punic alphabet, by mistake. As both those elements are not only visible, but tolerably well preserved, and as it were placed in contrast, on this coin, the truth of the point in question will the more clearly appear. I shall only beg leave to add, that if the piece was struck at Agrigentum, as I am inclined to believe it was, it must have been of a pretty early date, as the Carthaginians seem to have had no particular connections with that city for at least a century before the destruction of their republick; and that I am, with great consideration and esteem,

Good Sir,

Your very faithful,

and most obedient,

humble servant,

Christ Church, Oxon.

Nov. 12, 1768.

John Swinton.

(6) *Philosoph. Transact.* Vol. LIV. Tab. xxiv. p. 409.

XLI. Introduction to the following Observations, made by Messieurs Charles Mason and Jeremiah Dixon, for determining the Length of a Degree of Latitude, in the Provinces of Maryland and Pennsylvania, in North America ; by the Reverend Nevil Maskelyne, B. D. F. R. S. Astronomer Royal.

Read Nov. 24,
1768.

MESSIEURS Charles Mason and Jeremiah Dixon, who observed the last transit of Venus over the sun, at the Cape of Good Hope, under the direction of the Royal Society, had been since engaged, by the Right Honourable Lord Baltimore and the Honourable Mr. Penn, to settle the limits between the provinces of Maryland and Pennsylvania, in North America ; which they performed partly by trigonometrical, and partly by astronomical observations.

In the course of this work, they traced out and measured some lines lying in and near the meridian, and extended, in all, somewhat more than 100 miles ; and, for this purpose, the country in these parts being all over-grown with trees, large openings were cut through the woods, in the direction of the lines, which formed the straightest and most regular, as well as extensive vistas that, perhaps, ever were made.

Messieurs

Messieurs Maſon and Dixon perceived that a moſt inviting opportunity was here given for determining the length of a degree of latitude, from the meaſure of near a degree and half. Moreover, one remarkable circumſtance very much favoured the undertaking, which was, that the country, through which the lines run, was, for the moſt part, as level as if it had been laid out by art.

The astronomical obſervations had been taken with an excellent ſector of fix foot radius, conſtructed by Mr. Bird, the firſt which ever had the plumb-line paſſing over and biſecting a point at the centre of the inſtrument. This inſtrument was ſo exact, that they found they could trace out a parallel of latitude by it, without erring above 15 or 20 yards; in doing which, it ſhould be obſerved, that they generally uſed the ſame ſtars, commonly 6 or 8 or 10 in number, at the ſeveral ſtations, and made a double ſet of obſervations at each ſtation, with the limb of the ſector turned both to the eaſt and to the weſt. This ſector had been ſet up at the northermoſt point of the lines before-mentioned as proper for determining the length of a degree of latitude. In order to determine the difference of latitude between this point and the ſouthermoſt point of the lines, or the amplitude of an arch of a meridian contained between their parallels, it was neceſſary that the ſector ſhould be alſo ſet up at the ſouthermoſt point, and the like obſervations repeated there, upon the ſame ſtars, which had already been obſerved at the northermoſt point.

This plan of a meaſure of a degree in North America, Meſſieurs Maſon and Dixon ſubmitted to
the

the consideration of the Council of the Royal Society, and offered to carry it into execution, at the expence of the Society, if they thought proper. The Council determined that so useful and important a work should be completed, and accordingly sent out instructions to Messieurs Mason and Dixon for the regulation of their operations; particularly requiring them to measure the lines carefully over-again with fir-rods, which they sent to them, together with a brass standard, of 5 foot, with which the rods were to be compared frequently, and the difference noted, and also the height of the thermometer at the time; for the lines had been all measured before with a standard chain, which, though sufficient for the common purposes of surveying, was by no means to be depended upon in so nice an operation as that of measuring a degree of latitude. The Honourable Mr. Penn was pleased, at the request of the Royal Society, to grant the further use of his sector, before mentioned, and other instruments, to the observers, for completing this measure.

The method pursued in this work, is, that which the level disposition of the country pointed out. But the result may be expected to be more accurate on this account, as measures taken in a straight line, and on a level surface, are known to be capable of great exactness; and no adventitious errors are here introduced from any possible errors of a chain of triangles. Messieurs Mason and Dixon having also determined the angle which the oblique line made with the meridian, by proper astronomical observations, and the amplitude of the arch of their meridian line by several observations of zenith distances of fixed stars, made

made at both ends of the meridian, with the limb of the sector turned both east and west at each extremity ; this measure of a degree seems to me to be as well stated, and as much to be depended on, as any that has been made ; and will, I presume, be thought a valuable addition to the other measures of degrees, which have been taken with great care and pains, by various learned men, particularly the members of the Royal Academy of Sciences at Paris, who have acquired so much just reputation by their valuable labours bestowed on this subject.

It may not be improper to remark, that the level disposition of the country this degree passes through, which, as I understand, also obtains further to the south, and, in a great measure, to the north of the limits of the same, gives some advantage to this measure, with respect to the use that may be made of it in inquiring into the figure of the earth ; as there is no room for suspicion that the plumb-line of the sector could be deflected materially from its proper position by the attraction of any mountain, or even elevated ground of a more moderate height, continued for a great length ; which latter circumstance, not taken notice of before, the learned Father Bosovich has shewn, may produce a very considerable deviation of the plumb-line, in the elaborate treatise of the measure of a degree of the meridian between Rome and Rimini, taken by himself and his learned coadjutor, Father Le Maire.

XLII. *Observations for determining the Length of a Degree of Latitude in the Provinces of Maryland and Pennsylvania, in North America, by Messieurs Charles Maſon and Jeremiah Dixon.*

Read Nov. 24, 1768. **I**N this work, the first thing to be considered was, how to continue a right line : and this was done by setting up marks with the assistance of an equal altitude or transit instrument (for it was contrived so as to serve either purpose at pleasure), made by Mr. John Bird, of the same construction with that described by M. Le Monnier, in the preface to the single volume of the French *Histoire Céleste*.

The cylindrical ends of the cross axis of the telescope were laid in two angles of the supporters, which rose perpendicularly from a horizontal bar, that was fastened firmly to the upper part of the vertical axis. The axis of the telescope was set truly horizontal, by a spirit level hung on its cylindrical ends.

The brass frame, which receives the vertical axis, was screwed to a post fixed in the ground, in the direction of the line which was to be continued.

When the vertical wire in the telescope was brought to bisect any mark, it was kept in that direction, by confining firmly, between two pushing screws, a horizontal arm that projected from a collar that surrounded the vertical axis; and, to prove that a small shock would not alter its position, a small

pressure was applied against one of the supporters, which being removed, it was carefully noted, whether the wire returned again to bisect the mark.

At every station (or mark) the telescope was turned two or three times after the mark was fixed in the line, to prove that the said mark was truly set.—In general, the distances between the marks did not exceed a mile, nor were they less than half a one.

The telescope magnified about 25 times. Three or four marks were always left standing, and on a little rising ground they would all be seen in a right line, the vertical wire in the telescope bisecting their centers without sensible error.

The marks made use of in continuing the lines were concentric circles of black and white, painted upon both sides of a board 14 inches square. This board moved in mortices made in two posts, which were drove into the ground; and, when the center of the said mark was brought, by means of signals, into the line, it was fastened by wedges to the posts.

By means of a plummet, a peg was driven into the ground, and a notch cut in it, under the center of the said mark, in order to secure the line.

In the evening, when we left off, a mark was placed before, and two or three left behind us; and in the morning the instrument was again set up in the same place, to prove that the marks were not

The tremour of the air (caused by the sun's rays) was often very great; and, to avoid any error that might arise from the fluttering of the marks, we intermitted our operations sometimes for five or six hours

in a day, and were often obliged to make use of the morning or evening twilight.

In the continuation of the line, a person was left at the mark, behind the instrument, till another mark was set forward, to prove with a plummet that its center was not moved.

The vifo cut through the woods, in this work, was about eight or nine yards wide, and, in general, seen about two miles, beautifully terminating to the eye in a point.

The zenith distances of the stars, for determining the celestial arc, answering to the interval of the parallels of the northernmost and southernmost points of the lines, were made with an excellent sector of six foot radius, constructed by Mr. John Bird.

In the course of the work, for dividing the provinces of Maryland and Pennsylvania, the following lines were traced out, that offered themselves for determining the length of a degree of latitude.

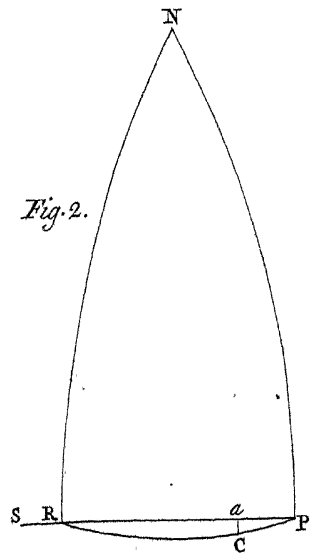
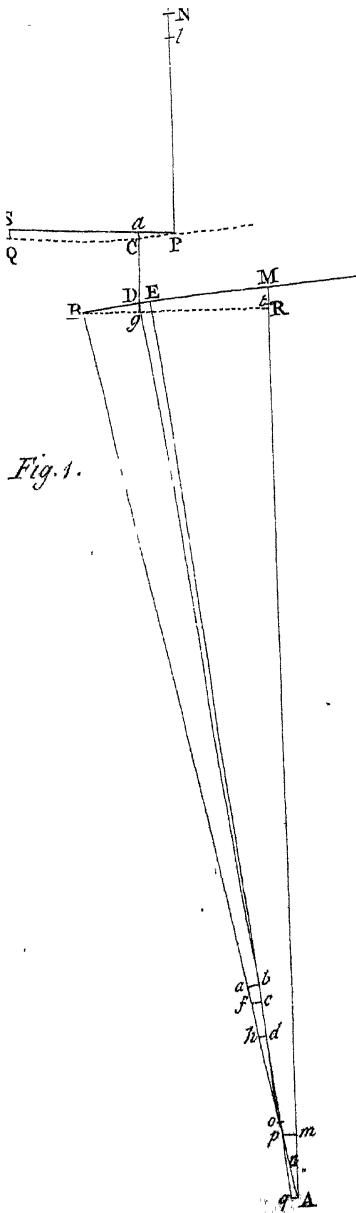
In the following fig. let N. represent the northernmost point, and A the most southern of the said lines. Beginning at N, a meridian was traced from N to

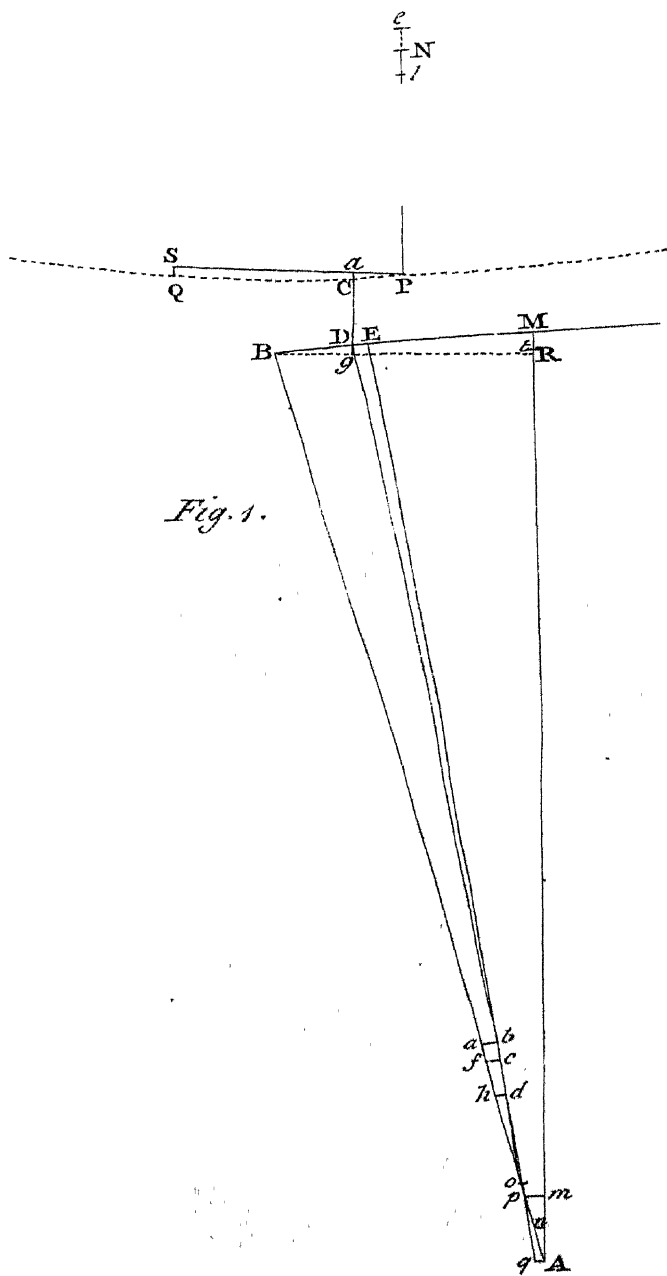
P. = ^{mi.} 14 ^{cha.} 64 ^{lin.} 8. In this line there were some hills, which were measured horizontally with a level, but the plains were measured with a chain.

PC = ^{m.} 2 ^{ch.} 79 ^{lin.} 27; C being in the parallel of latitude with P, which was determined by the sector.

DE a meridian = ^{m.} 5 ^{ch.} 2 ^{lin.} 43, in which are three or four small ascents and descents.

The





The points B D E and M are in a right line.
 $BD = 22 \overset{\text{cha.}}{51}$, and the angle $CDM = 86^\circ 32' \frac{1}{7}$
 nearly. Hence,

B is south of D $= 1 \overset{\text{ch.}}{36} \overset{\text{lin.}}{31} = Dg.$

The line $AB = 81 \overset{\text{ch.}}{78} \overset{\text{lin.}}{31}$, in which is one gentle rising hill, about half a mile over; all the rest of the line is an entire level or plain.

These measurements, expressed in English statute miles and parts of the same, were made with a chain, established from a brass statute yard, which was proved and corrected, in the course of the work, by another statute chain (kept only for that purpose) made from the said brass yard. They were only designed for dividing the provinces of Maryland and Pennsylvania: the same lines were re-measured afterwards with wooden rectangular levels, for the purpose of determining the length of a degree of latitude, as will appear in the sequel of this work.

The point C was placed in the parallel of latitude of P, thus. Let N (see TAB. XIII. fig. 2.) represent the north pole of the terrestrial globe; P and R two places lying in the same parallel of latitude RCP; PR an arch of a great circle $= 10'$ joining the said points; and PN, RN two meridians. PN or the complement of the latitude of P being $= 50^\circ 16' 42''$, the angle NPR or the azimuth of the great circle PR was found by calculation to be $89^\circ 55' 51''$. The going of the clock being found by equal altitudes of stars, the times were computed when the same or other stars would pass the azimuth of the line PR; and, at the time computed for any star, the intersection of the cross wires of the transit instrument being brought to

to cover the star, the telescope was turned down to the horizon, and a land-mark was fixed up at the distance of about half a mile, answering to the intersection of the wires. In like manner, by other stars, several other marks were fixed up, and the mean of all was taken. In this direction the line PR was continued; and though it was at first intended to extend it only to R, to the distance of 10' of a great circle, it was in fact prolonged somewhat further, to S, PS being = 12,312 miles, or 10' 45'' of a great circle. Now PC being = 2,991 miles, or 2' 37'' of a great circle, the angle NPC is = 89° 58' 55''; from whence NPS = 89° 55' 51'' being subtracted, there remains the angle SPC or aPC = 3' 4'', whence aC, or the distance of the parallel PCR at C, south of a, should be 14,1 feet. But it having been made a rule, in dividing the provinces of Pennsylvania and Maryland, to trace out the parallels of latitude by the observations taken with the astronomical sector only, the sector was put up at P and S successively (see fig. 1.) and the zenith distances of the stars Capella & Lyra, and others, were observed at both places; whence the point S was concluded to be 43 yards or 129 feet = SQ more northerly than P; and thence it was found by calculation, that the parallel of latitude PQ at the point C should be 45½ feet, = aC distant from the great circle PS, and to the south of the same; and the point C was placed accordingly, by laying off 45½ feet = aC, at right angles, to the line Pa from the point a towards the south.

aC found by the sector, being 45½ feet, and found by the azimuth of the line PS being 14,1 feet only,

it follows, that had the position of the point C been determined by the latter method, instead of the former, it would have been placed 31,4 feet more to the northward than it was found by the sector; and, in consequence, the length of the degree of latitude would have come out 21 feet longer. But the difference is so small, that it only serves to confirm the exactness of the work, and renders it unnecessary to enter into any consideration, which of the two methods ought to be preferred.

The meridians NP, CD, and AM, were found by celestial observations. The method of proceeding was as follows :

To find the meridian AM, and the angle that the line AB makes with the said meridian,

The equal altitude instrument being set up at the point A, with its vertical axis over the said point, equal altitudes of stars were observed for finding the motion of the clock. The time was next computed when some northern stars would pass the meridian by the clock, at which instant (shewn by the clock) the vertical wire in the telescope was brought to bisect the star; and, the vertical axis of the instrument remaining fixed, the telescope was turned down in the same azimuth to the horizon, and a candle placed opposite to the vertical wire, as a point in the meridian.

And the time of stars passing an azimuth in the direction of the line AB, for determining the angle BAM, was found by bringing the vertical wire in the telescope to bisect a candle placed (about $\frac{1}{4}$ mile from A) in the line AB; the telescope was then elevated to the star, and the time when it passed the said vertical wire taken.

The

The observations for determining the meridian A M, and the angle that the line A B makes with the said meridian, were as follows :

1766.		Time per clock.		Sum.		Half Sum.	
October.	h	m	s	h	m	h	m
b . . . 11	19	51	27	21	17	10	41
	52	49	—	18	35	+	41½
	54	13	—	19	56	—	35 42
				11	23		35 41½
				11	23		35 41½
Mean				—		20	35 42
						20	33 30
						2	12

} equal altitudes \propto Cygni.

star passed the meridian per clock.
star's apparent right ascension.

clock too fast for sidereal time.

O . . . 12		21 10 22::		
		h	m	h	m	h	m
	0 16	21	10	41	13	29	—
	1 42	—	—	13	30	—	—
	3 15	—	—	—	—	—	—
Mean		—		—		20	36 45
						20	36 44½
						20	33 30
						3	15

} equal altitudes \propto Cygni.

star passed per clock.
star's apparent right ascension.

clock too fast for sidereal time.

22 20 13 \propto Urfa major } passed an azimuth in the direction of the line A B.
23 26 β Urfa major }

1766- October.	Time per clock.		Sum.	Hal. Sum.	
	h	m		h	m
19 50 41	21	22	15 33	20	37 46½
51 58	23	35½	15 33½	37	47—
53 20½	24	52	15 33	31	46½
can				20	37 46½
				20	33 30

equal altitudes α Cygni

star passed per clock.
star's apparent right ascension.

Clock too fast. Hence the clock gained of sidereal time the last 24^h, 62'.

the line A B.

22 21 16 α Urfa major	} passed an azimuth in the direction c the line A B.		
22 24 21 β Urfa major			
		22 49 8	"
The apparent right ascension of α Urfa major		4	22—
The clock will be too fast, when this star will be on the meridian			
α Urfa will be on the meridian per clock		22 53 30—	
In like manner I find that γ Urfa major will be on the meridian per clock		23 45 55½	
And the pole * at		0 50 27	

At the instant when the clock shewed 22^h 53' 30", the vertical wire was brought to bisect the star α Urfa major; and then the vertical axis was made fast (the level shewing the horizontal position of the axis of the telescope and the line of collimation being just), the telescope was then brought down to the horizon, and by means of a candle seen through a small hole in a board, a mark, at the distance of 21 42, was placed in a line with the said vertical wire.

66.	Time	clock,	Sum	H: f Sum.
ber.	"	"	"	"
19	52 14+	21 23 47	41 18 42½	20 39 2 +
53	22½	25 10	18 42½	39 2 +
54	55½	20 28	18 42+	39 2
				20 39 +
				33 30
				<u>5 51+</u>

22 31 56 ♂ Urfa minor passed the line A B.
2 4 18 β Urfa minor passed ditto.

74 ... 16	20	0 17	21 17 24	41 20 32	20 40 16
		1 41	18 51½	20 32½	40 16 +
		3 8	20 16	20 33	40 16½
					<u>20 40 16 +</u>
					<u>20 33 30</u>
					<u>6 46 +</u>

} equal altitudes α Cygni.

clock too fast.

22 23 46 α Urfa major passed the line A B.
22 26 45½ β Urfa major. passed ditto.
22 32 45 ♂ Urfa minor passed ditto.

1786. October.	Time per clock.			Sum.			Ha	Sum.
	h	'	"	h	'	"		
23 41 16	23	41	16	0	6	37	0	3 18½
43 51½				0	6	37½		3 19—
48 51					6	38		3 19
				Mean	=	0	3 19	
							23 56 22	
							6 5	

} equal altitude of α Andromeda.

* passed the meridian per clock.
* 's apparent A R.

the clock too fast. Hence by the going of the clock, in the interval between α Cygni and α Andromeda: passing the meridian.

At	h	'	"	the Pole * will be on the meridian.
	0	53	0	β Urfa minor will be on ditto.
	2	58	41	
At	0	53	0	{ by the clock, the vertical wire was brought to the Pole * as usual; and, by means of a candle at the distance of a mile, a mark was placed, which fell, as near as could be judged, on the mark placed the 14th instant.
At	2	5	10	β Urfa minor passed the line AB.
	2	58	41	{ the wire was brought to β Urfa minor for finding a meridian as before; and, by means of a candle at the distance of a mile, a mark was placed, which fell 3 inches east of that placed the 14th.

N. B. In this last observation the axis of the telescope itself was turned upside down. This proved the ends of the cylinder to be good.

2 . . . 17 In the evening, by means of a candle placed behind a board, with a small hole in it over the mark placed the 13th infant, the line was extended to the marks at a mile distance, and there a mark placed, which fell $\frac{1}{4}$ of an inch east of the mark placed the 14th infant.

From the whole, there are six observations, all within the space of about 3 inches, at the distance of a mile : The mean was taken as a point in the meridian, north of the point A.

At this meridian point m , we laid off the line mp , at right angles to the meridian A, m , M, and, by a candle being placed at o , in the right line AB (about $1\frac{1}{4}$ miles from A), another candle was advanced along the line mp , till the vertical wire in the telescope bisected both candles : Under the candle, at the intersection of mp , with AB, viz. at p , a mark was placed in the ground.

The ground between m , and p , being made smooth (it was level as a floor by nature) the distance mp , was measured twice, and found to be 5 chains, 14 feet, and $\frac{1}{8}$ of an inch.

With this same chain the distance mA was measured \approx 80 chains exactly.

For the Angle BAM, by celestial measure.

	h	'	"			
The AR of the meridian when α Urfa major passed an azimuth in the direction of the line A B.	}	22	16	53, 8	by observations made	October 12.
		22	16	55, 1	D° on the	13.
		22	16	54, 7	D° on the	16.
Mean		22	16	54 5	=	334 13 38
AR of α Urfa major						162 16 54
Angle at the Pole =						171 56 44

AR of the meridian when β Urfa minor passed the direction of the line A B.		by observations made October 14.	
	h	'	"
	1 58	7, 8	
	1 58	14, 2	D° on the 15.
	1 58	7, 1	D° on the 16.
Mean of the first and last =	1 58	7, 5	29 31 52
AR of β Urfa minor			222 53 30
Angle of the Pole			166 38 22
Now having the distance of the *s from the Pole, the angles at the Pole, and the latitude of the point A, per spherics, we find the star's azimuth from the north = the angle B A M.			
And for the said Angle by terrestrial measurement. As A M : Rad :: $p m$:			
Tangent Angle $p A m$. =			3 43 40
Angle per α Urfa major			3 43 25
D° per β Urfa minor			3 43 25
Mean			3 43 30 = Angle B A M.

In the same manner as the point m , in the meridian from A was found, points in the meridian, north of P and N, were also found; and having two points given, with them a right line was extended as follows; first N P. in a line with N ϵ .—At N the equal altitude instrument was set up, and the vertical wire in the telescope was brought to bisect the mark at ϵ ; and there the vertical axis made fast. The spirit level shewing the axis of the telescope to be horizontal.

The vertical axis being well secured, the telescope part is taken off the supporters, and turned to point to the southward (carefully taking off, and putting it on the supporters so as not to move the axis); then on the farthest rifing ground that could be seen, another mark was placed at l_1 in a right line with the vertical wire. A mark being left at N, the instrument is taken, and set up three or four feet south of the mark l_1 , and having brought the vertical wire in the telescope in a right line with the marks at l_1 , and N, the vertical axis is then made fast as before, the telescope immediately turned, and a third mark placed to the southward; and so the operation was continued.

In

In the same manner the lines PS, CD, AB, and AE were traced out; and, to prove that by this method a right line may be extended, we shall here give the result of continuing the lines AB and AE. A and D being two points between which a right line was to be drawn.

The point n was known to be nearly in the line AD. At A and n marks were placed, and 3 or 4 feet north of n (which was $\frac{1}{4}$ of a mile from A), the instrument was set up; then, in the same manner as above, the vertical wire in the telescope was brought in a line with A and n , and the vertical axis made fast; the telescope was then turned to point to the northward, and a third mark placed, &c. &c. In this manner the line A n was continued to B.

Having continued the line to B, it fell 22 ^{chs.} 51, west of the point D: we then returned and laid off eastward off-fets from the line AB, at every fifth mile from A, proportional to the distance from A, and at the end of every off-fet placed a post, in order to form the line AD.

Off-fets were measured very correct at $\left\{ \begin{array}{l} \text{about } 10\frac{1}{2} \text{ miles } \dots \dots = ab. \\ \text{the } 10^{\text{th}} \text{ mile } \dots \dots = fc. \\ \text{about the } 9^{\text{th}} \text{ do } \dots \dots = bd. \end{array} \right\}$ and at the 3 points, b, c, d , three marks were placed. At b the instrument was set up, to see if the 3 marks were placed in a right line; when it appeared they were not exactly so; but, on moving the middle one $\frac{1}{4}$ an inch east, they then made a right line.

In this direction we continued the lines b, c, d , to A, which fell 2 feet 2 inches west of the point A, at q . The distance A q being so small a quantity (gradually rising) in ten miles, we thought it would be superfluous to change the direction, and therefore returned to the point b , and extended the line northward, proceeding in the same manner as before.

Having continued the line to E, it fell 16 feet 9 inches east of the point D. Hence the off-fets from this line to the true line AD, are as shewn by the Table B. And as we passed by the off-set posts made from AB, we measured the distance of this line AE from the said off-set posts, which were as given in the Table D.

TABLE D.

Miles from point A.	Off-sets in Feet	Inches	eastward
0	2	2	2
5	1	0	10
10	0	0	0
15	4	0	4
20	6	4	8
25	8	7	4
30	10	8	3
35	11	7	6
40	11	8	5
45	13	9	6
50	15	10	1
55	16	11	1
60	17	10	6
65	18	11	7
70	19	12	1
75	20	15	7
80	21	16	7
82	22	16	9

the distance of the off-set posts
west from the line A E.

and A E; that is, the off-set posts
the line (A E) traced last, that also
ows

Miles

TABLE B.

Miles from point A.	Off-sets in Feet	Inches	to the eastward.	to the westward, to give the true line A D.
0	2	2	2	
5	1	0	0	
10	0	2.2		
15	1	4		
20	2	6		
25	3	8		
30	4	10		
35	5	11		
40	7	1		
45	8	3		
50	9	5		
55	10	6		
60	11	8		
65	12	10		
70	14	0		
75	15	2		
80	16	4		
82	16	9		

From these Tables we have
the line A B, which is
the said line A D, will

Miles from point A.	Feet	Inches
0	0	0
5	0	2
10	0	2.2
15	1	0
20	2	2
25	3	8
30	3	5
35	1	7
40	1	4
45	1	3
50	1	0
55	0	7
60	1	2
65	1	3
70	1	1
75	0	5
80	0	3
82	0	0

This is a sufficient proof that the line AB is the arch of a great circle; but, as a farther confirmation that no error could arise, we observed at different points in the line the right ascensions of the meridian when the star δ Urfæ minoris passed an azimuth corresponding to the direction of the line, being in the upper part of its circle. The method of proceeding was in the same manner, as described before, for finding the angle BAM .

OBSERVATIONS for determining the right Ascensions of the Meridian, when δ Uræ Minoris
 passed the Line A B.

June 25, 1964, we began at the point A, to trace the line A B; and the weather being so cloudy prevented our making any one observation till July 10, though we attended every night. By this time we had continued the line 20 miles from A.

Time per watch.		Equal altitudes of Antares. Hence the * passed the meridian per watch at A R of Antares		Watch too fast for fiducial time	
h	m	h	m	h	m
1764.	0	17	5 10	16	23 36
July.	0	17	12 17	16	15 1
.....	10	15	49	16	18 35
	55	0	17		
	2	8	18 3		

20 Mi from A.

1764
July
20

290

Time per watch.

9	29	26	28	15	36	} equal alt. α Aquilæ.	Hence * passed at .	9 57 58
34	18	21	40				Right ascension of α Aquilæ	9 39 18
46	21	26	30					

Watch too fast . . . 0 8 40

22 44-45 δ Ursæ minoris passed an azimuth in the direction of the line A B

18 44 watch too fast when the * passed the line.

22 26 the right ascension of the meridian when δ Ursæ minoris passed the line A B.

0 45	19	45	4	} equal altitude α Lyræ.	Hence * passed at . . . 8 59 0
1 54	46	0			Right ascension . . . 8 28 58
2 56	47	1			Watch too fast . . . 0 30 2

33 58	21	30	50	} equal alt. α Cygni.	Hence * passed at . . . 21 3 58
35 33	32	23			Right ascension . . . 20 33 25
37 3	33	58			Watch too fast . . . 0 30 33

22 57 36 δ Ursæ minoris passed the direction of the line A B.

— 31 1 watch too fast at this time.

22 26 35 the right ascension of the meridian when δ Ursæ minoris passed the line A B

Time per clock.

1764.

July.

8 . . . 30

h	'	"	h	'	"
18	32	40	20	2	50
33	42
34	55	1	4	59	

equal alt. of α Lyrae.

Hence * passed at . . . 19 18 51
Right ascension . . . 18 28 58

Watch too fast . . . 0 49 58

46 Miles
from A.

20	56	12	21	49	13
57	56	50	52		
59	36	52	26		

Hence passed at . . . 21 24 23
* 's right ascension . . . 20 33 25

Watch too fast . . . 0 50 58

Watch too fast . . . 0 50 58

23 18 25 δ Urfæ minoris passed the direction of the line A B.
— 51 57 Watch too fast.

22 26 28 = the right ascension of the meridian when δ Urfæ minoris passed the line A B.

August.

. . . 17

18	47	25	19	0	0
48	24	37	0		
49	33	38	0		

equal alt. of α Lyrae.

Hence * passed at . . . 19 12 42
its right ascension . . . 18 28 58

Watch too fast . . . 0 43 44

11 Miles
from A.

20	42	45	21	50	48
44	8	52	12		
45	30	53	30		

equal alt. α Cygni.

Hence * passed at . . . 21 18 9
its right ascension . . . 20 33 25

Watch too fast . . . 0 44 44

Watch too fast . . . 0 44 44

23 12 48 δ Urfæ minoris passed the direction of the line A B.
— 45 39 = watch too fast.

22 27 9 = the right ascension of the meridian, when δ Urfæ minoris passed the direction of the line A B.

Time per watch.

1764.

August,

27

18	54	38	19	58	50
55	40	59	54		
56	44	20	0	57	

equal altitudes α Lyrae.

Hence the * passed merid. per watch, . . . 19 27 47
Right ascension of α Lyrae 18 28 36

Watch too fast 0 59 49

Here we
were 80
miles from
A.

21	6	32	21	54	46
8	4	56	25		
9	44	58	2		

equal alt. α Cygni.

Hence passed at 21 22 31
* 's right ascension 20 33 25

Watch too fast 0 59 6

23 25 55 δ Urfæ minoris passed the direction of the line A B.
— 59 21 watch too fast.

22 26 34 = the right ascension of the meridian when δ Urfæ minoris passed the line A B.

Most of these equal altitudes were observed per Mr. Dixon, I judging the time by the watch, which had only the hour and minute hands; therefore the seconds must not be expected as from a good clock, nor does the problem require it, as the star δ Urfæ minoris changed its azimuth very slowly. — The passage of the star δ Urfæ minoris over the line A B was in general taken by myself.

The A R of meridian when } 22 26 0 $\frac{1}{2}$ } by observations made on the 15th and 16th of October, 1766, at the point A.
δ Urfæ minoris passed A B. } 22 25 54 $\frac{2}{3}$

Whether the small effects of the aberration and nutation of the star δ Urfæ minoris, at the different times, will add to or diminish from the correspondence of these numbers, I have not determined; the above being sufficient for the purpose intended; for if the direction of the line had been changed any quantity of note, it would have caused a much greater difference in the right ascensions of the meridians, when the star passed the line, than any we here find.

The following are observations for determining the celestial arch between the points A and N. — Those marked with dots and *, thus, . . *, . were made by Mr. Dixon.

1766. We set up the sector at the point A, in the middle of a west line, drawn between Cape Hinlopen
 8 October. and Chisopeak Bay, and made the following observations.
 N. B. Each revolution of the micrometer = 52"

PLANE OF THE SECTOR EAST.

*'s names,	Nearest point on the sector,	Points on the micrometer,	Diff. between the points on the micrometer,	Apparent zen. distances.		
	0	'	0	"	0	' "
γ Andromedæ	2	45—	{ 5 31— 6 10—	0	31,3	2 44 28,7 North.
β Persei	1	35—	{ 7 8 7 14	0	6,0	1 34 54,0 N.
δ D°	8	35—	{ 7 32+ 9 13+ 6 16	1	25,0	8 33 35,0 N.
Capella	7	15+	{ 4 39— 3 24— 2 22½	1	21,3	7 16 21,3 N.
β Aurigæ	6	25+	{ 1 10 1 0½	1	3,2	6 26 3,2 N.
Castor	6	5—		0	9,5	6 4 50,5 South:

* Name,	Nearest point on the factor.	Points on the micrometer.	Diff. between the points on the mi- crometer.	°	'	"	North.
766. October.							
24 9 Cloudy,							
2 10	α Lyrae	3 26½ 0 43½	2 19,0	0	7	19,0	
	γ Cygni	4 42 6 6—	1 7,7	1	3	52,3	N.
	α D°	5 41 5 46—	0 4,7	5	59	55,3	N.
	γ Andromedæ	7 12 7 42	0 30,0	2	44	30,0	N.
	β Persei	8 15½ 8 22	0 6,5	1	34	53,5	N.
...*	δ D°	5 51½ 7 33	1 25,5	8	33	34,5	N.
	Capella	6 48½ 5 20½	1 20,0	7	16	20,0	N.
	β Aurigæ	3 29½ 2 17½	1 4,0	6	26	4,0	N.
	Castor	15 25 15 15½	0 9,5	6	4	50,5	N.

h	ii α Lyrae	8 26 5 41+	2 20,7	0	7	20,7	N.
	δ Cygni	5 44— 3 37—	1 51,0	6	6	51,0	N.
	γ D°	3 37 5 1	1 8,0	1	3	52,0	N.

*'s Names.	Nearest point on the factor.	Points on the micrometer.	Diff. between the points on the mi- crometer.	Apparent zen. distances.
1766. October.				
☉	8 35—	{ 1 43 3 26 6 28	0 1 27,0	8 33 33,0
♂ Persei	7 15+	{ 4 50 1 33+	1 22,0	7 16 22,0
Capella	6 25+	{ 0 21+ 15 37½ 15 29½	1 4,0	6 26 4,0
♂ Aurigæ	6 5—		0 8,0	6 4 52,0
Castor				

TURNED THE SECTOR PLANE WEST.

♂ 13	♂ Andromedæ	2 45—	{ 6 2+ 5 29½ 5 43	0 24,8	2 44 35,2
♂	♂ Persei	1 35+	{ 5 44— 6 2½ 4 24	0 0,7	1 35 0,7
♂	♂ D°	8 35—	{ 7 4½ 8 40	1 22,5	8 33 37,5
♂	Capella	7 15+		1 27,5	7 16 27,5

* & Names.	Nearest point on the factor.	Points on the micrometer.	Diff. between the points on the mi- crometer.	Apparent zen. distances.
1766, October.				
14 α Lyræ	5+	{ 14 12 16 49	2 21,0	0 7 21,0
α Cygni	0	{ 13 16 13 16	0 0,0	6 0 0,0
γ Andromedæ	45—	{ 2 46 2 22	0 24,0	2 44 36,0
β Persæi	35+	{ 2 5— 2 5+	0 0,6	1 35 0,6
δ D°	35—	{ 3 27+ 2 0—	1 19,6	8 33 40,4
Capella	15+	{ 1 50+ 3 36	1 29,7	7 16 29,7
β Aurigæ	25+	{ 2 51 4 18½	1 11,5	6 26 11,5
Castor	5—	{ 7 17 7 29	0 12,0	6 4 48,0
15 α Lyræ	5+	{ 3 33 6 20—	2 22,7	2 7 22,7
δ Cygni	5+	{ 6 18 8 32	1 58,0	6 6 58,0
γ D°	5—	{ 7 4 5 43—	1 5,3	1 3 54,7
α D°	0+	{ 6 36+ 6 37½	0 1,2	6 0 1,2
Andromedæ	45—	{ 6 40½ 6 16	0 24,5	2 44 35,5
β Persæi	35+	{ 5 48— 5 48—	0 0,0	1 35 0,0

1766. October.

Star	Nearest point on the fclot.	Points on the micrometer	Diff. between the points on the mi- crometer.			Apparent zen. d. francs.		
			'	"	°	'	"	°
15 δ Persei	8 35—	{ 8 2 6 24	1	22,0	8	33	38 0	
Capella	7 15+	{ 7 21 $\frac{1}{2}$ 9 5	1	27,5	7	16	27,5	
β Aurigæ	6 25+	{ 9 25— 10 44 $\frac{1}{2}$	1	11,8	6	26	11,8	
Castor	6 5—	{ 12 45+ 13 0	0	12,7	6	4	47 3	
<hr/>								
16 α Lyreæ	0 5+	{ 4 48+ 7 35	2	22,7	0	7	22,7	
δ Cygni	6 5+	{ 2 20 $\frac{1}{2}$ 4 32—	1	55,2	6	6	55,2	
γ D°	1 5—	{ 0 25 17 18 $\frac{1}{2}$	1	4 5	1	3	55,5	
α D°	6 0+	{ 0 8 $\frac{1}{2}$ 0 9	0	0,5	6	0	0,5	
γ Andromedæ	2 45—	{ 4 31 $\frac{1}{2}$ 4 33 $\frac{1}{2}$	0	28,0	2	44	32,0	
β Persei	1 35+	{ 3 25 3 25	0	0,0	1	35	0,0	
δ D°	8 35—	{ 2 24 $\frac{1}{2}$ 1 20—	1	23,5	8	33	36,5	
Capella	7 15+	{ 3 5 5 29 $\frac{1}{2}$	1	29,3	7	16	29,3	
β Aurigæ	6 25+	{ 6 48 8 5	1	10 5	6	26	10,5	
Castor	6 5—	{ 8 15 $\frac{1}{2}$	0	10,5	6	4	49,5	

* * * * *

*'s Names.	Nearest point on the factor.	Points on the micrometer.	Diff. between the points on the mi- crometer.		Apparent zen. distances.	
17 α Lyrae	0	5 18—	2	23,3	0	7 23,3
γ Cygni	1	8 5 8 1 $\frac{1}{2}$ 6 41 $\frac{1}{2}$	1	4,0	1	3 56,0
α D $^{\circ}$	6	6 48 $\frac{1}{2}$ 6 48	0	0,5	5	59 59,5
Capella	7	8 43 10 26 $\frac{1}{2}$	1	27,5	7	16 27,5
β Aurigæ	6	10 39— 12 6	1	11,3	6	26 11,3
Castor	6	12 14— 12 25+	0	11,6	6	4 48,4

18 α Lyrae	0	6 49 9 38+	2	25,3	0	7 25,3
δ Cygni	6	7 45— 10 8	1	59,3	6	6 59,3
γ D $^{\circ}$	1	5 15 4 3	1	4,0	1	3 56,0
α D $^{\circ}$	6	16 47— 16 49	0	2,3	6	0 2,3

THE RESULT OF THESE

Star's zenith distance at the point A.

PLANE OF THE

In the tent east.

	α Lyrae.	δ Cygni.	γ Cygni.	α Cygni.	γ Androm.
	D. ° ' "	D. ° ' "	D. ° ' "	D. ° ' "	D. ° ' "
1766. Oct.	10 0 7 19,0	11 6 6 51,0	10 1 3 52,3	11 6 0 5,5	8 2 44 28,7
	11 0 7 20,7	11 6 6 51,0	11 1 3 52,0	12 6 0 3,5	10 2 44 30,0
	12 0 7 22,0	12 6 6 51,0	12 1 3 51,0		11 2 44 30,7
					12 2 44 27,6
<hr/>					
Mean 11 11	0 7 20,57	6 6 51,0	1 3 51,7	6 0 4,50	2 44 29,25
Aberration	— 17,11	— 18,40	— 17,33	— 17,75	— 4,08
Nutation --	+ 6,12	+ 4,14	+ 2,92	+ 2,25	— 7,53
Precess. Oct.	0,0	0,0	0,0	0,0	0,0
11, 1764.					
Refract. ---	+ 0,12	+ 6,11	+ 1,06	+ 6,0	+ 2,75
<hr/>					
Mean zen. dist. the 11th Oct.	0 7 9,70	6 6 42,85	1 3 38,42	5 59 55,0	2 44 20,39

PLANE OF THE

October.					13 2 44 35,2
14 0 7 21,0			14 6 0 0,0		14 2 44 31,0
15 0 7 22,7	15 6 6 58,0	15 1 3 54,7	15 6 0 1,2	15 4 4 35,5	
16 0 7 22,7		16 1 3 55,5	16 6 0 0,5	16 --- --	
17 0 7 23,3		17 1 3 56,0	17 5 59 59,5		
-----	18 6 6 59,3	18 1 3 56,0	18 6 0 2,3		
<hr/>					
Mean ---	0 7 22,42	6 6 58,65	1 3 55,55	6 0 0,70	2 44 35,57
Aberration	— 16,74	— 18,31	— 17,40	— 17,93	— 4,79
Nutation --	+ 6,12	+ 4,14	+ 2,92	+ 2,25	— 7,53
Precess. ---	0,03	0,14	0,18	0,19	— 0,19
Refract. ---	+ 0,12	+ 6,11	+ 1,06	+ 6,0	+ 2,75
<hr/>					
Mean zen. dist. Oct. 11, 1766.	0 7 11,89	6 6 50,45	1 3 41,95	5 59 50,83	2 44 25,81
Plane east	0 7 9,70	6 6 42,85	1 3 38,42	5 59 55,0	2 44 20,39

Mean zen. dist. Oct. 11, 1766.	0 7 10,79	6 6 46,85	1 3 40,18	5 59 52,92	2 44 23,10
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OBSERVATIONS, AS FOLLOWS:

SECTOR EAST.

		β Persei.			δ Persei.			Capella.			β Aurigæ.			Castor.		
1766.	D.	°	'	"	°	'	"	°	'	"	°	'	"	°	'	"
Oct.	8	1	34	54,0	8	8	33	35,0	8	7	16	21,3	8	6	4	50,5
	10	1	34	53,5	10	8	33	34,5	10	7	16	20,0	10	6	4	50,5
	11	1	34	55,7					11	7	16	20,3	11	6	4	49,5
	12	1	34	55,0	12	8	35	33,0	12	7	16	22,0	12	6	4	52,0

	1	34	54,55		8	33	34,17		7	16	20,90		6	26	3,80		6	4	50,62
	—		1,09		+		2,0		+		5,48		+		6,55		—		3,52
	—		8,27		—		8,40		—		7,85		—		7,15		+		4,70
			0,0				0,0				0,0				0,0				0,0
	+		1,58		+		8,55		+		7,26		+		6,43		+		6,08

	1	34	46,77		8	33	36,32		7	16	25,79		6	26	9,63		6	4	57,88
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SECTOR WEST.

Octobre.

13	1	35	0,7	13	8	33	37,5	13	7	16	27,5									
14		35	0,6	14		33	40,4	14		16	29,7		14	6	26	11,5	14	6	4	48,0
15		35	0,0	15		33	38,0	15		16	27,5		15		26	11,8	15		4	47,3
16		35	0,0	16		33	36,5	16		16	29,3		16		26	10,5	16		4	49,5
								17		16	27,5		17		26	11,3	17		4	48,4

	1	35	0,33		8	33	38,10		7	16	28,30		6	26	11,28		6	4	48,30
—			1,79		+		1,20		+		4,98		+		6,22		—		3,75
—			8,27		—		8,40		—		7,85		—		7,15		+		4,70
—			0,16		—		0,14		—		0,07		—		0,02		—		0,10
+			1,58		+		8,55		+		7,26		+		6,43		+		6,08

	1	34	51,69		8	33	39,31		7	16	32,62		6	26	16,76		6	4	55,23
	1	34	46,77		8	33	36,32		7	16	25,79		6	26	9,63		6	4	57,88

	1	34	49,23		8	33	37,82		7	16	29,20		6	26	13,20		6	4	56,56
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1766.
December.

11 Set up the sector at the point N, in the forks of the river Brandwine, in Pennsylvania, and made the following observations.

*'s Names.	Nearest point on the sector.	Points on the micrometer.	Diff. between the points on the mi- crometer.	Apparent zen. distances.
γ Andromedæ	15+	{ 5 20 1 4 18 —	0 54,8	1 15 54,8 North.
β Persei	5+	{ 4 46 + 3 18 —	1 20,3	0 6 20,3 N.
δ D°	7 5+	{ 4 8 4 6 —	0 2,0	7 5 2,0 N.
Capella	5 50—	{ 5 35 8 17 —	2 18,0	5 47 42,0 N.
14 Cloudy.				
γ Andromedæ	15+	{ 7 32 6 29 —	0 55,0	1 15 55,0 N.
β Persei	5+	{ 8 3 — 6 25 —	1 21,7	0 6 21,7 N.
δ D°	7 5+	{ 6 27 6 24 —	0 3,0	7 5 3,0 N.
Capella	5 50—	{ 4 46 1 7 29 1 —	2 19,0	5 47 41,0 N.
β Aurigæ	4 55+	{ 9 20 1 6 29 1 —	2 27,0	4 57 27,0 N.
Cassio	7 35—	{ 9 14 1 7 35 —	1 23,5	7 33 36,5 South.

*'s Names.	Nearest point on the sector.	Points on the micrometer.	Diff. between the points on the mi- crometer.			Apparent zen. distances.		
	°	'	°	"	'	°	'	"
16 γ Andromedæ	1	15+	{ 9 37—	0	55 7	1	15	55 7 North.
β Perfei	0	5+	{ 8 33	1	21 0	0	6	21 0
δ D°	7	5+	{ 7 45½	0	20 0	7	5	20 0
Capella	5	50—	{ 6 16½	0	18 7	5	47	41 3
β Aurigæ	4	55+	{ 5 21½	2	27 7	4	57	27 7
Castor		35—	{ 5 19½	1	21 8	7	33	38 2
			{ 7 11					
			{ 9 46—					
			{ 9 39					
			{ 6 47+					
			{ 6 34½					
			{ 5 5—					
17 α Lyrae		20+	5 44—	1	42 0	1	21	42 0 Sou
18 Cloudy.			7 42—					
19 α Lyrae		20+	5 1½	1	43 5	1	21	43 5
β Perfei		5+	7 1	1	20 2	0	6	20 2
δ		5+	7 14+	0	3 7	7	5	3 7
Capella	5	50—	7 4	2	16 0	5	47	44 0
β Aurigæ		55+	5 42½	2	27 3	4	57	27 3
Castor		35—	8 22½	1	21 3	7	33	38 7
			8 6+					
			5 15					
			4 45+					
			3 16					

Diff. between the
points on the mi-
crometer, distances.

nearest point Points on the
on the sector, micrometer,

*'s Names.

20 Cloudy.

21 α Lyrae { 3 2 0 — 1 20+ { 3 2 0 — 1 41,7 1 21 41,7

TURNED THE SECTOR PLANE WEST.

21 β Persei { 6 4+ 1 29,7 0 6 29,7

δ D° { 7 42 0 10,3 7 5 10,3

Capella { 7 50 — 2 56,8 5 47 56,8

β Aurigae { 4 5 — 2 7,2 5 47 52,3

Castor { 7 0 — 2 35,3 4 57 35,3

22 Cloudy.

23 D° { 9 51 1 30 7 33 30,0

24 α Lyrae { 10 0+ 1 36,0 1 21 36,0

γ Andromedæ { 11 38+ 1 4,8 1 16 4,8

β Persei { 6 44 — 1 30,2 0 6 30,2

δ D° { 5 0 — 0 10,5 7 5 10,5

* Names.		Nearest point on the factor.	Points on the micrometer.	Diff. between the points on the mi- crometer.			Apparent zen. distances.		
		°	'	9	19	'	"	°	'
1766. Decemb.									
g	24	5	50—	{	9 19—	2	7,7	5	47 52,3
					6 47				
					6 2				
...		4	55+	{	9 1	2	35,0	4	57 35,0
					6 31+				
		7	35—	{	8 16½	1	29,2	7	33 30,8
u	25								
q	26								
b	27	1	20+	{	5 13+	1	35,3	1	21 35,3
					3 22				
		1	15+	{	8 19	1	4,0	1	16 4,0
					9 31				
		0	5+	{	9 17½	1	29,5	0	6 29,5
					11 3				
		7	5+	{	12 16+	0	9,7	7	5 9,7
					12 26				
		5	50—	{	6 32	2	9,0	5	47 51,0
					4 7				
o	28	1	15+	{	4 14—	1	3,7	1	16 3,7
					5 26—				
		0	5+	{	2 42½	1	28,5	0	6 28,5
					4 27				
		4	55+	{	8 14—	2	35,6	4	57 35,6
					11 13+				
					R				

PLANE OF THE

1766. December.	α Lyræ.		γ Androm.		β Persei.	
	o	' "	o	' "	o	' "
	13	1 15 54,8	13	0 6 20,3	
	15	1 15 55,0	15	0 6 21,7	
	16	1 15 55,7	16	0 6 21,0	
	17	1 21 42,0	
	19	1 21 43,5	19	0 6 20,2	
	21	1 21 41,7	
Mean	1 21	42,40	1 15 55,17		0 6 20,80	
Aberration	+	2,20	— 11,76		— 9,20	
Deviation	—	5,66	— 7,60		— 8,19	
Precess.	+	0,48	— 3,12		— 2,64	
Refraction	+	1,36	+	1,26	+	0,10
Mean zen. dist. Oct. 11, 1766.	1 21	40,78	1 15 33,95		0 6 0,87	

PLANE OF THE

1766. December.		21 0 6 29,7	
	24	1 21 36,0	24	1 16 4,8	24	6 30,2
	27	21 35,3	27	16 4,0	27	6 29,5
	28	16 3,7	28	6 28,5	
Mean	1 21	35,65	1 16 4,17		0 6 29,48	
Aberration	+	0,15	— 11,63		— 9,56	
Deviation	—	5,66	— 7,60		— 8,19	
Precess.	+	0,53	— 3,76		— 3,05	
Refraction	+	1,36	+	1,26	+	0,10
Mean zen. dist. Oct. 11, 1766	1 21	32,03	1 15 42,44		0 6 8,78	
D° Plane East	1 21	40,78	1 15 33,95		0 6 0,87	
True mean zen. dist. 11 Oct. 1766, at the point N. } D° at the point A.	1 21	36,42	1 15 38,19		0 6 4,87	
	0 7	10,79	2 44 23,10		1 34 49,23	
Difference	1 28	47,21	1 28 44,91		1 28 44,40	
	28	44,91				
		44,40				
		43,98				
		45,09				
		44,34				
Mean	= 1 28 44,99 = the true celestial arch between the points N and A.					

SECTOR EAST.

δ Persei.			Capella.			β Aurigæ.			Castor.		
°	'	"	°	'	"	°	'	"	°	'	"
13	7	5 2,0	13	5 47 42,0							
15		5 3,0	15	47 41,0	15	4 57 27,0	15	7 33 36,5			
16		5 2,0	16	47 41,3	16	57 27,7	16	7 33 38,2			
19		5 3,7	19	47 44,0	19	57 27,3	19	33 38,7			
<hr/>											
	7	5 2,67		5 47 42,08		4 57 27,33		7 33 37,80			
	—	8,61		— 3,20		— 0,60		— 3,78			
	—	8,26		— 7,52		— 6,75		— 4,20			
	—	2,26		— 0,95		— 0,29		— 1,25			
	+	7,08		+	5,80	+	4,95	+	7,55		
<hr/>											
	7	4 50,62		5 47 36,21		4 57 24,64		7 33 44,52			

SECTOR WEST.

21	7	5 10,3	21	5 47 52,8	21	4 57 35,3	21	7 33 30,0
24	7	5 10,5	24	5 47 52,3	24	57 35,0	24	33 30,8
27		5 9,7	27	47 51,0	
.....				28	57 35,6	
<hr/>								
	7	5 10,17		5 47 52,03		4 57 35,30		7 33 30,40
—		9,37	—	4,24	—	1,60	—	3,52
—		8,26	—	7,52	—	6,75	+	4,20
—		2,56	—	1,09	—	0,32	—	1,36
+		7,08	+	5,80	+	4,95	+	7,55
<hr/>								
	7	4 57,06		5 47 44,98		4 57 31,58		7 33 37,27
	7	4 50,62		5 47 36,21		4 57 24,64		7 33 44,52
<hr/>								
	7	4 53,84		5 47 40,60		4 57 28,11		7 33 40,90
	8	33 37,82		7 16 29,29		6 26 13,20		6 4 56,56
<hr/>								
	1	28 43,98		1 28 48,60		1 28 45,09		1 28 44,34

this being a
little wide of
the rest is
left out.

The following are Observations made at the Points N, and near P, in the Year 1764 :
But the Length of Time between these, and those made at A, being near three Years—
probably the Set made at N, in December 1766, may be best to be used in determining
the Length of a Degree of Latitude.

STAR'S ZENITH DISTANCES.

SECTOR IN THE TENT, PLANE EAST.

1764.	γ Andromedæ.			β Persei.			α Persei.	δ Persei.							
	o	'	"	o	'	"		o	'	"					
January.	17	1	15	0,0	0	5	40,0	7	4	29,5 ^u				
	19	0	5	39,6	7	4	31,0				
	20	1	15	2,2	0	5	39,0	9 3	47,0	7 4	30,2				
	21	1	15	1,3	0	5	37,5	9 3	49,7	7 4	30,8				
	22	1	15	1,2	0	5	38,0	7	4	31,0				
Mean, January	20	1	15	1,2	0	5	38,8	9 3	48,3	7 4	30,5				
Aberration declin.	—			10,0	—		9,1	—	11,4	—	10,4				
Deviation D°	—			3,3	—		5,7	—	6,2	—	6,7				
Precess. fr. 1 Jan. 1764.	—			1,0	—		0,8	—	0,7	—	0,7				
Refraction	+			1,4	+		0,1	+	10,5	+	8,3				
Observatory S° of Tent	+			0,3	+		0,3	+	0,3	+	0,3				
Mean zen. diff. 1 Jan. 1764		1	14	48,6		0	5	23,6		9	3	40,8	7	4	21,3

SECTOR IN THE OBSERVATORY, PLANE WEST.

January	26	1	15	4,7	0	5	41,7	7	4	32,8		
27	27	1	15	5,7	0	5	41,0	9	3	57,0		
28	28	1	15	4,5	0	5	44,0	9	3	57,5	7	4	33,0
29	29	1	15	5,5	0	5	44,0	9	3	55,3	7	4	32,7
Mean, January	27½	1	15	5,1	0	5	42,7	9	3	56,6	7	4	32,8
Aberration in Declin.	—	—	9,0	—	8,6	—	11,1	—	10,3				
Deviation D°	—	—	3,3	—	5,7	—	6,2	—	6,7				
Precess. from 1 Jan. 1764	—	—	1,3	—	1,1	—	1,0	—	0,9				
Refraction	+	+	1,4	+	0,1	+	10,5	+	8,3				
Mean zen. dist. 1 Jan. 1764		1	14	52,9	0	5	27,4	9	3	48,8	7	4	23,2
D° Plane East		1	14	48,6	0	5	23,6	9	3	40,8	7	4	21,3
True zen. dist. at the point N, 1 Jan. 1764.	}	1	14	50,8	0	5	25,5	9	3	44,8	7	4	22,2
Precess. to Oct. 11, 1766		+		49,45		40,56	+		34,61			
Reduced to Oct. 11, 1766		1	15	40,25	0	6	6,06	7	4	56,81		

*'s Z. DIST. AT THE POINT N.

PLANE WEST.

	Capella				β Aurigæ.				Castor				α Lyrae.				
		o	'	''		o	'	''				Jan.	o	'	''		
1764.	27	5	47	46,8	28	4	57	38,4	27	1	21	57,5				
	28	5	47	47,0	29	4	57	36,4	28	1	21	57,0				
January.	29		47	46,7	Feb. 2.		57	38,0								
February.	2		47	47,8				Feb.		1	21	57,3			
	3		47	44,7				3	7	33	6,6	Ab.	—	9,5		
				5	4	57	36,8	5	7	33	4,8	Dev.	—	9,4		
				6	4	57	36,2	6	7	33	5,5	Prec.	+	0,2		
				8	33	6,8	Refr.	+	1,5			
											Feb.	1	21	40,1		
											11	1	22	3,8		
											13	1	22	1,3		
Mean	30	5	47	46,6	1	4	57	37,2	5 $\frac{1}{2}$	7	33	5,9	12	1	22	2,5	
Aberration in Declin.	—			7,4	—			5,7	—			0,6	—			13,0	
Deviation	—			8,8	—			9,2	+			9,1	—			9,4	
Precels. from 1 Jan. 1764	—			0,4	—			0,1	—			0,7	+			0,3	
Refraction	+			6,7	+			5,8	+			8,8	+			1,5	
Meanzen. diff. 1 Jan. 1764		5	47	36,7		4	57	28,0		7	33	22,5		1	21	41,9	
														1	21	40,1	
														Mean	1	21	41,0

PLANE EAST.

	February	Feb.	Feb.
							16	1	22	8,2							
							18	4	57	35,7							
	20	5	47	39,0	20	4	57	34,7	20	7	33	7,3	20	1	22	10,5	
	21	5	47	38,7	21		57	35,0	21	7	33	6,3	21	1	22	8,8	
	22		47	38,0	22		57	35,2	22		33	5,7	22	1	22	9,8	
													26	1	22	10,8	
Mean	21	5	47	38,6	21	4	57	35,2	21	7	33	6,4	22	1	22	9,6	
Aberration	—			8,0	—			6,9	+			0,5	—			14,8	
Deviation	—			8,8	—			9,2	+			9,1	—			9,4	
Precels. from 1 Jan.	—			0,7	—			0,2	—			0,95	+			0,35	
Refraction	+			6,7	+			5,8	+			8,8	+			1,5	
Meanzen. diff. 1 Jan. 1764		5	47	27,8		4	57	24,7		7	33	23,8		1	21	47,3	
D° Plane West		5	47	36,7		4	57	28,0		7	33	22,5		1	21	41,0	
True zen. diff. at the point } N, r Jan. 1764		5	47	32,3		4	57	26,3		7	33	23,1		1	21	44,2	
Precels. to Oct. 11, 1766		+		14,56		+		4,28		+		18,97		—		7,03	
		5	47	46,86		4	57	30,58		7	33	42,07		1	21	37,17	

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Stars Zenith Distances observed at a Point 7 chains 91 links North of P.

PLANE EAST.

CAPPELLA.												α Lyræ.												δ Cygni.												γ Cygni.												α Cygni.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																															
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Star's Zenith Distances observed at a Point 7 chains 91 links North of P.

PLANE WEST.

1764.	Capella.	α Lyræ.	δ Cygni.	γ Cygni.	α Cygni.
May 28	6 0 32,7	27 1 08 59,0	27 4 50 14,8	26 0 12 52,6	26 4 43 4,3
June 1	0 32,3	28 8 59,5	28 4 50 51,0	27 12 51,0	27 43 4,2
5	0 31,7	June 3	8 58,0	June 28	12 51,0
		4	8 59,8	3	12 51,0
		6	8 58,2	4	12 50,3
		7	8 57,3	5	12 49,8
		8	8 57,3	6	12 50,0
				7	12 49,0
				8	12 48,0
Mean	1 6 0 32,2	3 1 8 58,4	3 4 50 15,9	3 0 12 50,3	3 4 43 6,2
Aberration	+ 1,4	— 6,3	+ 10,4	— 11,3	+ 12,8
Nutation	— 9,0	— 9,4	+ 8,74	— 8,1	+ 7,6
Precess. from 1 Jan. 1764	— 2,2	+ 1,1	— 3,40	+ 4,6	— 5,2
Refraction	+ 7,0	+ 1,2	+ 5,6	+ 0,2	+ 5,5
Mean zen. diff. 1 Jan. 1764 at a point 7 ch. 91 lin. Dr. Plane East	6 0 29,4	1 8 45,0	4 50 37,24	0 12 35,7	4 43 26,9
True zen. diff. 1 Jan. 1764 at a point 7 ch. 91 lin. North of P	6 0 26,60	1 8 47,00	4 50 35,19	0 12 38,20	4 43 25,95
Precess. to 11 Oct. 1766	+ 14,56	— 7,93	+ 23,03	— 30,75	+ 34,59
True zen. diff. 11 Oct. 1766	6 0 41,16	1 8 39,97	4 50 58,22	0 12 7,45	4 44 0,54
Dr. at the point A	7 16 29,20	+ 7 10,79	6 6 46,65	1 3 40,18	5 59 52,92
Difference	1 15 48,04 15 50,76 48,43 47,63 52,38	1 15 50,76	1 15 48,43	1 15 47,63	1 15 52,38
Mean	15 49,45 = the celestial arch between the point 7 ch. 91 lin. N. of P and the point A				

Remarks on re-measuring the Lines with two rectangular Levels, or measuring Frames.

The levels used in this work were, each, 20 feet in length, and 4 feet in height. They were made of pine, an inch thick, and in form of a rectangle; the breadth of the bottom board was $7\frac{1}{2}$ inches, that of the top = 3 inches, of the ends = $4\frac{1}{2}$ inches, and the bottom and top were strengthened with boards firmly fixed to them at right angles. The joints were secured with plates of iron, and the ends were plated with brass. The plumb lines used in setting them level, were = 3 feet and 2 inches in length, and hung in the middle of the levels, being secured in a tube from the wind, in the manner of carpenters levels; wherefore we called these by the same name.

When the plumb-line bisected a point at the bottom, the ends were perpendicular.

Where the ground was not horizontal, or there were logs, &c. to pass over, one end of the level was raised by a winch and pulley.

The level being set, a short staff was drove into the ground (very near and opposite the plumb-line), in the top of which moved a thin plate of iron, about 12 inches long; at the ends of which were points, which were directed to the intersections of lines, drawn on the board that covered the plumb-line. By bringing the points in a line with one of the said intersections, if the level was by accident moved, it might be discovered, and brought again to its place.

A level being thus marked, the end of the other was brought in contact with it, and marked in the same manner, before the first was moved; the first was then taken up, and set before the last. And so the operation was continued. Mr. Dixon attended one plumb-line and staff, and I the other. The measure was carried on in a straight line, and in the proper direction, by pointing the levels to the farthest part of the vista that could be seen; this was readily and accurately done, on account of their lengths. The levels were frequently compared with the brass standard, of 5 feet, provided for that purpose, and the difference was noted between 8 times the brass standard, and the length of the two levels taken together; as may be seen in the 3d and 4th columns of the following table. This difference serves for reducing the measure taken with the levels, to what it would have been if it had been taken with the brass standard itself; see column 6th. For facilitating this comparison of the levels with the brass standard, pieces of brass were fixed into the bottom boards of the levels, on each of which was drawn a faint line. And one tenth of an inch at the end of the brass standard being divided into ten parts or hundredths of an inch, the difference between eight times the brass standard, and the two levels joined together, was with the help of a magnifying glass of a short focus, determined to great accuracy. Moreover, the brass standard being liable to alter with the changes of heat and cold, a further correction becomes necessary on that account, in order

order to reduce the measures to the temperature of 62° of Fahrenheit's thermometer, which is the term to which the former operations of this kind have been reduced. For this purpose, the rate of expansion of brass is taken from Mr. Smeaton's experiments, made with a pyrometer of his invention (see Philos. Transf. Vol. XLVIII. Part II.) which is $\frac{232}{100000}$ th of an inch upon a length of one foot for a variation of 180° of Fahrenheit's thermometer; whence the expansion answering to four times the length of the brass standard, or 20 feet, or the length of one level, would be $\frac{464}{100000}$ th of an inch for the same difference of the thermometer; and $\frac{258}{1000000}$ th of an inch for 1° of the same thermometer. Therefore, in order to find the correction of column 7th, the constant quantity, .00258 was multiplied by the difference of 62, and the degree of height of the thermometer; and that product, again multiplied by the number of levels measured, gave the correction required in inches and decimal parts of an inch; which was additive or subtractive, according as the thermometer was higher or lower than 62 degrees.

In the following Process, the

1. and 2. Columns contain the time of the day, M signifying morning. A afternoon.
3. - - - the height of the thermometer at D°.
4. - - - the quantity, in hundredth parts of an inch, that the two levels, taken together, were more or less than eight times the brass standard, or 40 feet.
5. - - - the number of levels measured between the times, that the levels themselves were measured with the brass standard.
6. - - - the corrections, or quantity in inches, to be added to, or subtracted from, the number of levels measured each day, arising from the levels being more or less than the brass standard.
7. - - - the correction, or quantity arising from the thermometer in inches.

Began, at the point N, to re-measure the lines with two rectangular levels, 20 feet each in length.

1768	N	2	3	4	5	6	7
February							
d 23	M	52	+ .12	203		+ 42.18	4.97
	A	53	+ .12				
	The breadth of Brandwine the 1st time			8.18		+ 0.49	- 0.47
	D° a 2d, where we crossed			12.20		+ 0.73	

1768	1	2	3	4	5	6	7	
February								
			D' a third time		9,87	+ 0,39	- 0,20	
24	9	M	54	+ ,08				
	11½	A	44	+ ,12	325	+ 16,25	- 13,41	
	5		39	+ ,32 ::				
25								
26	9½	M	40	+ ,12				
	4½	A	45	+ ,11	247	+ 14,08	- 12,42	
27								Rain.
28								
29	8½	M	40	+ ,08	80			Rain in the afternoon.
						+ 3,20	- 0,41	
March 1	9½	M	32	+ ,23				
	4½	A	42	+ ,21	320	+ 35,20	- 20,64	
2	8½	M	32	+ ,20	200	+ 15,10	- 10,58	
	1	A	51	+ ,10	170	+ 9,09	- 5,48	
	5½		48	+ ,115				
3	8½	M	40	+ ,185				
	2	A	48	+ ,125	230	+ 17,82	- 10,68	
	5		48	+ ,145	120	+ 8,10	- 4,33	
4	8½	M	31	+ ,19	200	+ 17,50	- 14,19	
	2	A	38	+ ,16	150	+ 12,90	- 10,83	
	5½		30	+ ,185				
5	8	M	27	+ ,18	220	+ 16,50	- 15,89	
	1	A	41	+ ,12	120	+ 8,40	- 8,36	
	5½		29	+ ,165				
6								
7	8½	M	28	+ ,155	250	+ 15,37	- 18,70	Very dry winds with frost.
	2	A	38	+ ,09	90	+ 4,41	- 5,80	
	5½		36	+ ,105				
8	8	M	36	+ ,103	230	+ 3,8	- 10,68	
	1½	A	52	- ,037	170	- 1,02	- 5,92	
			45	+ ,06				

[315]

1768 March	1	2	3	4	5	6	7	
9	8 $\frac{1}{2}$	M	51	+,065	270	— 3,10	— 2,44	Here subtract 4 inches, for going round the corner of a barn.
	2	A	66	—, 11	110	— 2,58	— 0,85	
	5 $\frac{1}{2}$		52	+,015				

10	8 $\frac{1}{2}$	M	58	—,01	179,2	— 1,79	— 1,15
	0 $\frac{1}{2}$	A	61	—,03			

This reached the point P.

Hence NP = 3914,45 levels + 199,02 inches — 178,40 inches = 78290,72 feet.

Began at the Point C.

2	11	11	M	60	—,04	137,15	— 2,74	— 0,71	Rain.
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h	12	9	M	52	+,07	108	+ 2,16	— 1,95
		1	A	58	+,01	156	+ 3,51	— 2,61
		5 $\frac{1}{2}$		53	+,08			

○ 13

24 17 Began where we left off, on the 12th instant, to measure as before.

	10	M	34	+,09	132	+ 5,94	— 8,51
	2	A	40	+,09	144	+ 6,12	— 8,91
	5		36	+,08			

	18	8 $\frac{1}{2}$	M	36	+,085	264	+ 7,65	— 15,66
		2	A	42	+,03	156	+ 4,06	— 9,45
		5 $\frac{1}{2}$		35	+,073			

	19	9	M	36	+,115	233,35	+ 14,93	— 17,15
		Noon	31	+,145				

This reached the point D.

Hence CD = 1330,50 levels + 41,63 inches — 64,95 inches = 26608,06 feet.

From D to g = 1 chain, and 36 links found before; = 89,76 feet.

○ 20 Sent to Philadelphia, about 40 miles distant, for tents, &c.

24 24 Began at the point B, to measure the line A B.

	1 $\frac{1}{2}$	M	49	—,08	132	— 5,28	— 4,42
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25

	26	Noon	57	—,05	211,05	+ 1,58	— 6,53
		6	A	43	+,065		

○ 27

[316]

1768	1	2	3	4	5	6	7	
March	1							
28								Snow.
29	8	M	40	+,19	264	+,18,48	-12,60	Compared two thermometers; they agreed within one division.
	2	A	47	+,09	264	+,13, 2	-12,60	
	5½		40	+,11				
30	8½	M	38	+,14	288	+,11,95	-9,28	
	2½	A	61	+,025	192	+ 5,95	-4,46	
	6¼		45	+,10				
31	8½	M	45	+,15	204	+,12,85	-4,21	By accident one of the thermom. was broke.
	2	A	62½	+,105	192	+,13,15	-2,97	
	6½		49	+,17				
April								
1	8	M	39	+,195	324	+,31,10	-13,79	
	2	A	52	+,19	204	+,19,38	-9,73	
	6		35	+,19				
2	9	M	40	+,18	228	+,18,24	-11,17	
	2½	A	46	+,14	168	+,12,18	-8,88	
	6¼		37	+,15				
3								
4	9½	M	38	+,04	312	+,12,48	-14,08	Snow in the evening.
	3	A	51	+,12	192	+,12,00	-9,16	
	6		36	+,13				
5								Snow.
6	8½	M	37	+,14	228	+,15,39	-10,58	
	3	A	51	+,13				
	6¼		38	+,13	204	+,13,26	-10,70	
Crossed Bohemia river obliquely =					33,875	+,2,21		
7								Snow all day, and frost at night.
8	11	M	37	+,23	396	+,38,02	-24,01	
	6½	A	40	+,155				

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1768	1	2	3	4	5	6	7
April	h						
9	9	M	44	+165	192	+ 7,96	- 5,69
	2	A	57	CO	204	+ 1,02	- 4,21
	6		51	+1,02			

10	9	M	59	-1,017	288	-12,81	+ 0,37
	2½	A	66	-1,16	228	-14,82	+ 0,29
	6½		59	-1,10			

12	8	M	47	-1,02	348	-13,92	- 3,14
	3	A	70	-1,14	156	- 6,63	+ 1,21
	6		60	-1,03			

13	9	M	64	-1,08	240	-11,16	+ 4,95
	1	A	76	-1,105	156	- 6,86	+ 3,22
	6		64	-1,07			

14	8½	M	53	-1,02	396	-11,98	- 9,10
	4½	A	53	-1,00			

15	9	M	45	+1,18	264	+23,76	-11,57
Rain in the afternoon: the day we pass through water two feet deep for half a mile							

16	8	M	46	+1,20	396	+39,99	-11,23
	4	A	56	+1,205			

17	8½	M	52	+1,11½	264	+ 6,20	- 1,70
	Noon		67	-1,02	192	- 1,44	+ 0,24
	6½	A	58	-1,01			

19	8½	M	53	+1,01	444	- 9,50	+ 2,23
	3	A	74	-1,075	132		
		after 3 ^h aft.					

20	8½	M	57	+1,025	312	- 3,12	+ 4,43
	2½	A	78	-1,065	256	- 1,12	+ 1,18
	6½		61	-1,02			

[318]

768 April	I h	2	3	4	5	6	7	
21	8½ 1½	M A	52 75	+ ,06 + ,02	264			
			aft. 1½ A		132	+ 7.92	+ 1.53	Left off in a swamp of water 18 inches deep.

22 }								Rain day and night.
23 }								

24								Rain till 11 ^h A.M.
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25 }								Swamps so full of water we could not proceed.
26 }								

27	10 5½	M A	73 72	00 - ,035	420	- 3.78	+ 11.38	
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28	8½ 6	M A	54 61½	- ,03 - ,02	396	- 4.95	- 4.09	
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29	
30	7 2 6	M A	60 76 73	+ ,19 45.11 + ,11	336 108	+ 25.20 + 5.94	+ 5.20 + 3.47	

ay	I	2	8½ 6½	M A	54 56	+ ,05 + ,09	384	+ 13.44	- 6.93
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3	10 3 6½	M A	61 82 75½	+ ,115 + ,02 45.075	288 108	+ 9.79 + 2.59	+ 7.06 + 4.74	This day we found the diff. between the 42d and 43d mile posts: 3 levels and 6 feet more than usual between 2 mile posts.
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4	10½	M	70	+ ,115	228	+ 13.18	+ 10.00	Thunderstorm all the morning: passed the main branch of Choptank.
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1768 May	1	2	3	4	5	6	7
24	5	8 2 5½	M A	70½ 86 72	+,115 +,02 +,09	264 132	+ 8.97 + 3.63 + 5.79
25	6	9½ 4	M A aft. 4 ^h A	66 63 72	+,14 +,155 72	324	+ 29.11 + 2.55
26	7	8 3	M A	60 74	+,185 +,065	408	+ 25.5 + 5.26
27	8						
28	9	7½ 4½	M A	63 85	+,03 -,095	528	- 8.69 + 16.35
29	10	8½ 7	M A	61 68	+,005 +,01	372	+ 1.49 + 2.40
30	11	9½ 6½	M A	70 72	+,111 +,06	528	+ 22.44 + 12.26 Rain. in the night and morning.
31	12	7 2 6½	M A	54½ 67 68	+,23 +,03 +,015	264 276	+ 17.16 + 3.03 - 0.68 + 3.92 Rain at night.
1	13	8½ 3	M A aft. 3 ^h A	68 75	+,10 -,025	300 96	+ 7.33 + 9.71
2	14	10½ 3	M A	66 74½	+,145 +,14	264	+ 18.74 + 5.45
3	15						
4	16	7½ 3½	M A aft. 3½ ^h A	57 81	+,19 -,02	384 144	+ 22.44 + 9.53
5	17	8½ 3	M A aft. 3 ^h A	66 87	+,12 -,085	384 144	+ 4.75 + 19.75

1768 May		P	2	3	4	5	6	7	
8	18	8½ 1	M A	67 90	+ ,085 + ,14	144	+ 8,13	+ 6,13	Puffed through Marthy Hope, water 4 or 5 feet deep.
24	19	9½ 3	M A	69 86	+ ,15 + ,075	264	+ 14,78	+ 10,55	
2	20	8 3	M A	69 93	+ ,17 + ,04	276 168			Great dews for 4 morn- ings past.
			aft. 3½ A				+ 23,31	+ 21,76	
h	21	9 4	M A	73 86	+ ,08 — ,01	360	+ 6,30	+ 16,25	Great dew.
0	22								
D	23								Rain.
8	24	7 3	M A	50 75	+ ,35 + ,21	480 180			
			aft. 3½ A				+ 92,40	+ 0,85	
2	25	9½ 4	M A	59 56	+ ,28 + ,34	396	+ 61,38	— 4,59	Rain last night and this morn- ing. Puffed through water part of the day.
24	26	9½ 5½	M A	58 53	+ ,28 + ,36	528	+ 97,68	— 8,85	Rain last night and part this day, the levels continually wet.
2	27	8 3½	M A	65 70	+ ,18 + ,14	396 264			Dry weather.
			aft. 3½ A				+ 52,80	+ 17,62	
5	28								
0	29								
2	30								

1768 May	1 h	2	3	4	5	6	7	
♂	3 ^r	8 $\frac{1}{4}$ 3 $\frac{1}{2}$	M A	79 90	+ ,01 — ,12	376	— 10,34	+ 21,82
								Very dry weather for 3 days past.

This reach'd to the North side of the river Nanticoke, near to the 7th mile post; here we left a mark.

24 June	2	Passed over the river Nanticoke, and began at the 6th mile post from the point A.						
	8	M	84	+ ,075	300			
	2 $\frac{1}{2}$	A	74	+ ,08				
		aft. 2 $\frac{1}{2}$ h A			228	+ 20,59	+ 23,15	
♀	3	9	M	76	+ ,155	396		
		3	A	85	+ ,02			
			aft. 3 ^h A		264	+ 29,04	+ 31,50	
h	4	6 $\frac{1}{4}$ 1	M A	64 82	+ ,213 00	398,875	+ 21,15	+ 11,32 At the point A.

⊙	5	Began at the 6th mile post, and measured Northward through the swamp of Nanticoke.						
2	7 $\frac{1}{2}$	M	67	+ ,12	230	+ 10,12	} to the river 6,83	
	2	A	77	+ ,055				
		Cross the river to the mark left the 31st of May.			34,67	+ 1,54		

This finished the line A B.
Hence AB = 21696,47 levels + 892,34 inches + 94,51 inches = 434011,64 feet. { The breadth of the rivers was found by measuring a base, and taking angles with a Hadley's quadrant.

Note. The reckoning was kept by stretching a rope in the line to be measured (in general) = 12 levels, which was often proved: and it was almost impossible that an error could arise; as we always began the rope with the same level, and ended it with the other; the rope not being removed till the last level was set.

The person that stretched the rope, sometimes Mr. Dixon, and sometimes myself, kept the account of the number of ropes measured: though the mile posts in the lines AB and DC were sufficient for that purpose, as the lines had been so often measured before.

In the line NP there were no mile posts, but two or three intermediate marks, which we found to agree in a general law with the levels.

Supposing the levels exactly = 20 feet each; then in the line NP a mile per chain measure = a mile and 9,44 feet by the levels; and in the line CD a mile per chain measure = a mile and 9,86 feet by the levels.

In the line AB, what the levels make more than the chain, between the mile posts, as follows.

Miles from the point A.	Diff. in feet.	Miles from the point A.	Diff. in feet.	<div> <p>Here appears to be an error of one chain = 66 feet, in the chain mea- sure, as observed before. p. 318.</p> </div>
81	14 $\frac{1}{2}$	43	74	
80	30 $\frac{1}{2}$	42	16	
78	25	40	8	
76	14	39	14	
75	43	38	14 $\frac{1}{2}$	
72	32 $\frac{1}{2}$	37	14 $\frac{1}{2}$	
70	61	36	14 $\frac{1}{2}$	
66	15—	35	14 $\frac{1}{2}$	
65	33+	34	14 $\frac{1}{2}$	
63	33 $\frac{1}{2}$	33	14 $\frac{1}{2}$	
61	16 $\frac{1}{2}$	28	79 $\frac{1}{2}$	
60	15 $\frac{1}{2}$	27	15	
59	10 $\frac{1}{2}$	25	32	
58	14 $\frac{1}{2}$	24	20 $\frac{1}{2}$	
57	15	23	14	
56	13	22	11 $\frac{1}{2}$	
55	12 $\frac{1}{2}$	21	12 $\frac{1}{2}$	
54	10 $\frac{1}{2}$	19	22	
53	12 $\frac{1}{2}$	18	14 $\frac{1}{2}$	
52	11	15	29	
51	9 $\frac{1}{2}$	13	14 $\frac{1}{2}$	
50	9 $\frac{1}{2}$	12	5	
49	17 $\frac{1}{2}$	10	11 $\frac{1}{2}$	
48	7	9	8	
47	9 $\frac{1}{2}$	8	7 $\frac{1}{2}$	
45	9 $\frac{1}{2}$	6	14	
44	7	4	23	
43	9 $\frac{1}{2}$	2	13	
		0	21 $\frac{1}{2}$	

We took notice of these differences as we measured from B to A, always finding the miles greater by the chain measure, by the quantity above, which shews that the chain was continually extending itself by use; as we had direct proof of, being obliged to contract it every day, and re-adjust it to its proper length by means of the standard chain.

To find the Latitude of the Point N.

	δ Persei			Capella			β Aurigæ			Castor			α Lyrae		
	°	'	"	°	'	"	°	'	"	°	'	"	°	'	"
True observed zenith dist. reduced to 1 Jan. 1764.	7	4	22,2	5	47	32,3	4	57	26,3	7	33	23,1	1	21	44,2
Stars decl. by Dr. Bradley.	47	0	40,0	45	43	53,0	44	53	44,2	32	22	56,8	38	34	34,0
Latitude by the different stars.	39	56	17,8		56	20,7		56	17,9		56	19,9		56	18,2
			20,7												
			17 9												
			19,9												
			18,2												
Mean =	39	56	18,9	= the latitude of the point N.											
Arch between N and A =		1	28 44,9												
	38	27	34	= the latitude of the point A.											
	39	11	56	= the mean latitude.											

Cha. Mafon.
Jere. Dixon.

The Length of a Degree of Latitude in the Province of Maryland and Pennsylvania, deduced from the foregoing Operations; by the Astronomer Royal.

THE difference of latitude of the points N and A, or the amplitude of the celestial arch, answering to the distance between the parallels of latitude passing through N and A, has been found by the sector, page 306, to be $1^{\circ} 28' 45''$. The terrestrial measure of the distance of the said parallels is next to be found: This is composed of the sum of the lines NP, CD, Dg, and AR, the last mentioned line being the reduction of AB to a meridian line passing through A: therefore BR expresses a parallel of latitude passing through B. Let Bt be an arch of a great circle drawn perpendicular to the meridian line, AR produced. The triangle BA t, on account of the smallness of its sides with respect to the radius of the earth, and the smallness of the angle BA t $= 3^{\circ} 43' 30''$ may be taken for a plane rectilinear triangle; in what follows, without any sensible error, as will appear to any one who makes the trial. Therefore it will be, by proportion, as radius is to the cosine of the angle BA t $= 3^{\circ} 43' 30''$ so is AB $= 434011,6$ English feet, to At $= 433694,6$ English feet. But this is to be lessened by the small quantity Rt, or the distance of the parallel circle BR from the great circle

t , which is to a third proportional to the diameter of the earth and the line BR , as the tangent of the latitude of the point B , to the radius. Whence $Rt = 15,8$ feet which subtracted from At just found =

433094,6 leaves	$AR =$	433078 8 feet
To which add { as found before {	$NP =$	78290,7
	$CD =$	26608 0
	$Dg =$	89,7

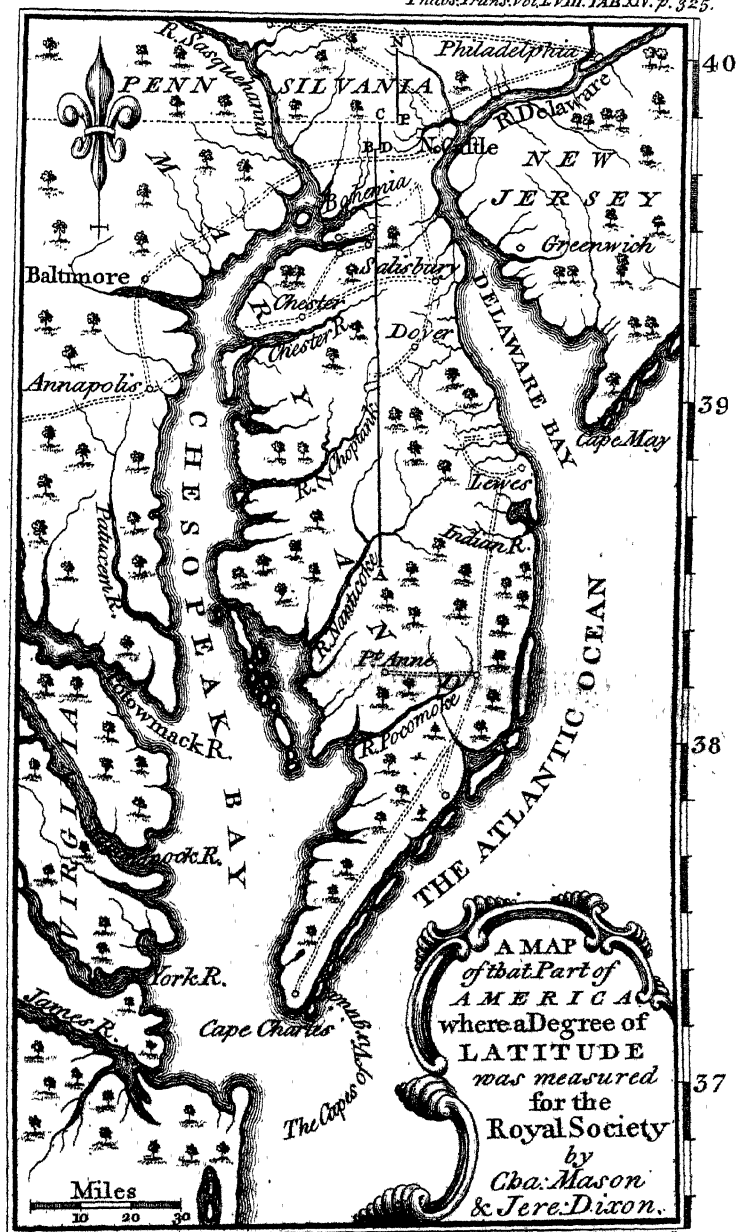
The sum is = 538067 feet

an arch of meridian intercepted between the parallels of latitude passing through the points N and A , answering to the celestial arch $1^{\circ} 28' 45''$.

Then say, as $1^{\circ} 28' 45''$; is to $1^{\circ} ::$ so is 538067 feet, to 363763 English feet, which is the length of a degree of latitude in the provinces of Pennsylvania and Maryland. The latitude of the Northernmost point N , was determined from the zenith distances of several stars, see page 323 = $39^{\circ} 56' 19''$ and the latitude of the Southernmost point $A = 38^{\circ} 27' 34''$. Therefore the mean latitude expressed in degrees and minutes is = $39^{\circ} 12'$.

To reduce this measure of a degree to the measure of the Paris toise, it must be reminded, that the measure of the French foot was found upon a very accurate comparison, made by Mr. Graham, of the toise of the Royal Academy of Sciences at Paris, with the Royal Society's brass standard, to be to the English foot, as 114 to 107. See Philosophical Transact, Vol. XLII. p. 185. Therefore say as 114 : is to 107 :: so is 363763 the measure of the degree in English feet, to 341427 the measure of the degree in French feet, which divided by 6, the number of feet in a toise, gives the length of the degree = $56904\frac{1}{2}$ Paris toises, in the latitude $39^{\circ} 12'$ North.

Such is the length of a degree in this latitude, supposing the five feet brass standard made use of in this measure to have been exactly adjusted to the length of the Royal Society's brass standard. It was really adjusted by Mr. Bird, by his accurate brass scale of equal parts, which he makes such excellent use of in dividing astronomical instruments, and which is just $\frac{1}{100000}$ th part of an inch shorter than the Royal Society's brass standard upon a length of three feet. If one would take notice of so small a difference, the length of a degree just found must be lessened by $\frac{1}{100000}$ th part, or by ten feet, in order to reduce it to the measure of the Royal Society's standard. Since I am treating of such niceties, may it be allowed me to add, that the five feet brass standard having been again compared with Mr. Bird's scale, since its return from North America, appeared both to myself and Mr. Bird to be just $\frac{1}{100000}$ th part of an inch shorter than the scale, upon that side on which the hundredths of an inch are placed at one end, and $\frac{2}{100000}$ ths of an inch shorter than the scale upon the opposite side? which diminution of its length is undoubtedly owing to the small wearing or battering which it has met with in the frequent use that was made of it. But the divided side of the rod having been that which was made use of in measuring the levels, is what is to be regarded in the present case. If one would allow for the wearing of the rod, one may suppose it to have suffered a gradual diminution; and then one must take a mean between its first length, which was the same with Mr. Bird's scale, and its present length, which is $\frac{1}{100000}$ th of an inch less; as one may suppose it a medium to have been $\frac{1}{100000}$ th part of an inch shorter than



than Mr. Bird's scale; on which account the length of the degree should be further diminished by $\frac{1}{133000}$ th part, or 3 feet, which added to 10 feet, the correction required on account of the difference of Mr. Bird's scale and the Royal Society's standard, gives 13 feet to be subtracted from the length of the degree calculated above. The whole correction will perhaps be thought scarce deserving of notice, especially as an error of only 1" in the celestial measure would produce an error of no less than 67 feet in the length of the degree. Moreover it is probable that the length of a degree has been already taken 10 or 20 feet too short, by placing the point C. too far to the Southward; which would about balance the small correction in question. Therefore, all things being considered, the length of the degree may be stated as given above, viz. = 363763 English feet or 56904½ Paris toises. It must, however, be observed, that the accuracy of this reduction into Paris toises depends upon a supposition that the length of the French toise, which is of iron, was laid off by the gentlemen of the Royal Academy of Sciences, upon the brass rod sent over to them for that purpose by Mr. Graham (which was afterwards returned to him); in a room where the heat of the air answered to 62 of Fahrenheit's thermometer, or 15 of Reaumur's, or nearly so, which is probable enough, but is a point that does not appear to have been ascertained. For, on account of the difference of expansion of brass and iron; 2 rods made of those metals, however accurately they may be made of equal lengths at first, will only agree together afterwards in the same temperature of the air in which they were originally adjusted together. It is fortunate that the uncertainty in the present case is but small, since 20° difference of Fahrenheit's thermometer or 10° of Reaumur's produces, according to Mr. Smeaton's experiments, a difference of the expansions of brass and iron of only $\frac{1}{133000}$ th part, which would cause an error of only 27 English feet or about 4 Paris toises in the length of the degree.

It is however to be wished, that the proportion of lengths of the French and English measures might be again ascertained by another careful experiment, in which the temperature of the air, as shewn by the thermometer, might be noted at the time.

[See the Map of the Country, where the foregoing Observations were made, Tab. XIV.]

POSTSCRIPT, BY THE ASTRONOMER ROYAL.

HAVING, some time ago, acquainted M. De la Lande, of the Royal Academy of Sciences at Paris, by letter, of this measure of a degree of latitude in North America; and at the same time expressed my doubts about the certainty of reducing it to French measure, from the proportion of the English to the French foot found by Mr. Graham; principally because no notice had been taken of the height of the thermometer at Paris, when the length of the French iron toise was laid off upon the brass rod sent thither by Mr. Graham, whence the proportion of the two measures was afterwards determined by him; and having also mentioned my opinion of the expediency of making another experiment of the proportion of the two measures, in which every necessary circumstance should be noted; and that I might probably request the favour of M. De la Lande to take the trouble to cause a French toise to be made for me, and to see it exactly adjusted to their standard, and then sent to me; he has been pleased to lend me two toises,

at the same time touching the other cheek, the moveable cheek was screwed fast; and thus the toise was exactly contained between the cheeks without any shake, and it is evident that the interval between the cheeks was exactly equal to the length of the toise. In order to measure this interval, the toise being taken away, very fine lines were drawn with a fine point, at the end of each cheek, upon the brass pins which were in the same plain with the board: then the cheeks were removed, and fine points made at the outer extremity of each line, and this distance being taken between the fine points of a beam compass, was transferred to the scale, and thus the length of the toise was found in measures of the scale, which is divided by a vernier to thousandths of an inch. The toises and brass scale had been left together in the same room, and near one another all the night before, and till the very time of making the comparison of the toises with the scale, in order to be sure that they were all affected with the same degree of heat.

As it may be agreeable to the reader to see the result of the principal measures of degrees of latitude, that have been taken with later instruments and proper accuracy, brought together into one view, the following table is here added:

Length of a degree in Paris toises.	Mean latitude.	Names of the observers.	Years in which the degrees were measured.
57422	56° 20' N	M. de Maupertuis, &c.	1736 and 1737
57074	49 23 N	M. de Maupertuis, &c. and M. Cassini	1739 and 1740
57091	47 40 N	P. Liesganig	1768
57028	45 0 N	M. Cassini	1739 and 1740
57069	44 44 N	P. Beccaria.	1768
56979	43 0 N	Le Pere Boscovich and Le Maire	1752
56888	39 12 N	Mess. Mason and Dixon	1764 to 1768
56750	0 0	M. Bouguer and M. de la Condamine	1736 to 1743
57037	133 18	Stabbe de La Caille	1752

If this degree be compared with the degree measured at the equator = 56750 toises, in the hypothesis of the earth's being an oblate spheroid, the ratio of the equatorial to the polar diameter will come out as 494 to 493. But, if it be compared with the degree measured in Lapland, in the latitude 66° 20' = 57419 toises (I have subtracted 3 toises, because the toise used in Lapland was $\frac{1}{10}$ th or $\frac{1}{12}$ th of a line less than the toise used in Peru, see M. De la Lande's *Astronomy*, Article 2107), the ratio of the diameters will be as 142 to 141. The great difference of these results is a fresh proof of what has appeared from the comparison of the measures of the several degrees taken before, either that the figures of the meridians are not accurately elliptical, or that the inequalities of the Earth's surface have a considerable effect in deflecting the plumb-line from its true situation, or both. I had indeed supposed that any deflections of the plumb-line were not to be feared with respect to this particular measure of a degree, at the end of my Introduction to Messieurs Mason and Dixon's account of the same, by arguing

arguing, perhaps too far, from the level disposition of the country through which the degree passes. But the Honourable Mr. Henry Cavendish has since considered this matter more minutely; and having mathematically investigated several rules for finding the attraction of the inequalities of the Earth, has, upon probable suppositions of the distance and height of the Allegany mountains from the degree measured, and the depth and declivity of the Atlantic ocean, computed what alteration might be produced in the length of the degree, from the attraction of the said hills, and the defect of attraction of the Atlantic; and finds the degree may have been diminished by 60 or 100 toises from these causes. He has also found, by similar calculations, that the degrees measured in Italy, and at the Cape of Good Hope, may be very sensibly affected by the attraction of hills, and defect of the attraction of the Mediterranean Sea and Indian Ocean.

The rules, which I used in calculating the ratio of the equatorial diameter to the polar axis, from the North American degree, compared with those measured in Peru and Lapland, are those given by Mr. John Robertson, Librarian to the Royal Society, in his *Elements of Navigation*, p. 597, as deduced by him from Dr. Letherland's *Geometrical Analysis* of the problem, which he has also given to the public in the same place, together with some other problems depending upon it, which were necessary to complete the subject,

1767

Time per Clock.

January.	d	h	'	"	h	'	"	
8	7	3	50	26	5	55	8 $\frac{1}{2}$	} Equal altitudes of Capella.
			51	39+		56	27	
			52	56 $\frac{1}{2}$		57	41 $\frac{1}{2}$	
14	8	4	4	48	5	40	3	} Equal altitudes of ditto.
			6	7 $\frac{1}{2}$		41	24	
			7	30—		42	44 $\frac{1}{2}$	

The first Satellite of Jupiter immersed.
 Apparent time 8 17 42 47 $\frac{1}{2}$

h.	10	{	6	21	28	8	2	45	} Equal altitudes of Castor.
..*	..	{		22	43		4	3	
..*	..	{		24	00		5	18—	

The first Satellite of Jupiter immersed.
 Apparent time 10 12 10 23.

9	16	{	4	5	4+	5	35	23	} Equal altitudes of Capella.
..*	..	{		6	25		36	47	
..*	..	{		7	50		38	7	

18	19	{	4	4	8 $\frac{1}{2}$	5	34	44	} Equal altitudes of ditto.
..*	..	{		5	27 $\frac{1}{2}$		36	8	
..*	..	{		6	52		37	29	

8	27	{	3	32	53	6	2	7	} Equal altitudes of ditto.
		{		34	5—		3	21 $\frac{1}{2}$	
		{		35	19 $\frac{1}{2}$		4	34 $\frac{1}{2}$	

February.

8	3	{	4	21	12 $\frac{1}{2}$	6	36	10 $\frac{1}{2}$	} Equal altitudes of β Aurigæ . . Windy.
..*	..	{		22	22		37	24	
..*	..	{		23	35+		38	34	

8	4	{	3	34	52—	5	56	0 $\frac{1}{2}$	} Equal altitudes of Capella.
		{		36	5		57	16+	
		{		37	20		58	29	

8	8	{	3	55	32	5	33	5 $\frac{1}{2}$	} Equal altitudes of ditto.
..*	..	{		56	30+		34	26+	
..*	..	{		58	12		35	45 $\frac{1}{2}$	

The first Satellite of Jupiter was not immersed } flying clouds.
 Ditto was immersed.

8	23	{	5	7	9	5	7	9	} Equal altitudes of Capella.
		{		8	53		8	53	
		{		10	32+		10	32+	

The first Satellite of Jupiter immersed. Ap. time 25^d 12^h 24^m 45^s . . .
 From

From these observations we have the time of Capella's passing the meridian, and the rate of the clock's going as follows:

		* passed merid.		Clock loses of Sid. time per day.	Mean rate of therm.	1764	D eclipsed	
		per clock.				March.	Time per watch.	
1766	Decemb.	h	' "	"	o	h	' "	
	24	4	57 40+	16.3	35	h 17	8 4 10	Eclipse of the D
	28		55 35	18.0	23			ended.
1767	30		55 59	13.4	6			h " "
January.	1		55 32+	14.8	37	8	58 46 10 27 30::	} Equal altitud. of Re- gulus.
	7		54 3	17.0	20	9	1 16 29 41	
	8		53 46	16.3	37	4	5 32 9	
	16		51 36	16.0	31	The watch went very regular sider. time.		
	19		50 48+	15.63	33	—Hence the eclipse ended at 8 ^h 21' 59"		
	27		48 43+	15.35	28			
February.	4		46 40½	15.5	30			
	8		45 38½	15.9	35	app. time, in the forks of the river Bran- diwine.		
	25		41 8—					

N. B. The edge of the earth's shadow on the D's disk was the best defined I ever saw: it was remarkably distinct from the penumbral shade.

N. B. The clock was firmly screwed to a piece of timber, 22 inches in breadth, and five inches and a quarter thick; the said piece of timber was let four feet into the ground, which was composed of a very firm, dry, hard clay.

The clock was placed in a tent, with Fahrenheit's thermometer hung to its side; and a blanket was wrapped round the clock and thermometer, to secure it from any wind that might enter the tent. The pendulum was adjusted to the upper scratch, with N^o 3. at the Index, as directed by the Rev. Mr. Maskelyne, Astronomer Royal: but the spring at the suspension of the pendulum having been broke, (when the ship, in which it was sent, was wrecked on the Jersey coast) we cannot be certain that the pendulum is now of the same length as it was when sent from London.

Those observations marked : are a little dubious; those marked :: are very dubious; those marked . . * were made per Mr. Dixon. The eclipses of J^l's satellites were observed with a reflecting telescope of one foot focus, that magnified about 70 times.

1766	Decemb. ^a	Height of the		Height of the		Vibration of	
		ther. at about 7 ^h	in the mor. in the	ther. at about 2 ^h	in the after. in the		
		Tent	Air	Tent	Air	on each side of O.	
						that is, half	
						the arch of vi-	
						bration.	
	24	43	45—	o	
	25	44	46		
	26	38	37	45	47	1	40—
	27	38	41	40	42		
	28	21	18	31	26	1	35
						Near midn. the therm. in { Tent 20	
						the { Air 16	
	29			28	28	At 10½ P. M. therm. { Tent 29	
						in the { Air 28	
	30			32	32	Near midn. in the Tent 17 Air 14	
	31	5 above O. 3 below O.		18	20	both above O.	

1766
Decemb. d

3¹ At 8^h $\frac{1}{4}$ P. M. ther. in the tent at O. in the air at 7 below O.
 1767 At 10^h $\frac{1}{4}$ P. M. ditto in the tent at 3 below O. in the air at 13 below O.
 Jan. 1 At 7^h 6' in the morn, the ther. in the $\left\{ \begin{array}{l} \text{Tent } 10 \\ \text{Air } 20 \end{array} \right\}$ below O.
 At 8^h 45' P. M. ther. in the $\left\{ \begin{array}{l} \text{Tent } 21 \\ \text{Air } 17 \end{array} \right\}$ above O. Vibration = 1° 12'
 At 11^h 4' P. M ditto in the $\left\{ \begin{array}{l} \text{Tent} = 3 \\ \text{Air} = 12 \end{array} \right\}$ below O Vibration = 1 10

2 At 6^h 42' in the morn. ther. in the $\left\{ \begin{array}{l} \text{Tent } 9 \\ \text{Air } 22 \end{array} \right\}$ below O.

At 10 ditto vibration of the pendulum = $1^{\circ} 5'$ on each side of O.
The pendulum now swings a little farther on the west side of O. than
on the east side. The clock faces the north.

21 15 1° 7' At 9 P. M. In the { Tent 9
Air 5

3	11	9			
4	before	34	39	39	1° 20'
5	37-	37	48	49	1° 35'
6	49		53	54	1° 40'

{ Pend. now swings rather far-
theft on the east side of O.

At 8½ P. M. in the { Tent 43
Air 44

7 At 11^h P. M. in the tent 25, in the air 26.

8 28 at 10 $\frac{1}{2}$ P. M. ther. in the tent 23.

9 17 40 40 1° 35' {Pend. wings 8' more on the
east side of O. than on the w.
43 At midn. ther. in the air 25.

II 50 47

I2

At 4 P. M. in the { Tent 42
Air 44

42 45 1° 40

The pend. swings as on the 9th.

23 33 33

39 41

1767
January d

	Height of the ther. at about 7 ^h in the mor. in the Tent Air	Height of the ther. at about 2 ^h in the aft. in the Tent Air	Half the arch of vibration.
--	--	--	--------------------------------

16 30 30 39 37 1° 35' The pend. swings as before.

At 9^h 5 $\frac{1}{2}$ P. M. ther. in the { Tent 24
Air 21

I 7 at 9^h A. M. $\left\{ \begin{array}{l} 28 \text{ tent} \\ 25 \text{ air} \end{array} \right.$ 43 39

18 33 34 39-39

19 25 26 39 36 At 9^h $\frac{1}{2}$ P. M. ther. in the Tent 21
Air 18

20 39 40

21 39 39 40 40

22 23 21 27 27 1° 30' { The pendulum swings to the eastward as before.

$$23 \quad 25 \quad 23 \quad 32 \quad 32$$

24 32 32 43 40 1° 30' Wound up the clock.

15	32	32	31	30
----	----	----	----	----

26 28 27 At 4^h $\frac{1}{2}$ P. M. ther. in the { Tent 32
Air 32.

At 4^h $\frac{1}{2}$ P. M. in the { Tent 27
Air 25

9 ditto .	{ Tent 15
	{ Air . 12

28 11 14 36 32 1° 20' The pendulum swings as before.

20 15 13 35 34

29	15	15	34	34
30	16	16	31	35

31 32 35 At 4^h $\frac{1}{2}$ P. M. in the { Tent 36
Air 36

Feb. I 36 35 36 37

2 15 13 40 3

3 16 15 41 38

At 9^h $\frac{1}{2}$ P. M. in the { Tent 26
Air 25

4 14 10 34 32 1° 30'

At 9^h P. M. in the { Tent 24
Air 23

5 30 32 45 4

6 13 12 28- 24

7 13 12 34 36

1767 Febr.	d	Height of the ther. at about 7 ^h in the mor. in the		Height of the ther. at about 2 ^h in the aft. in the		Half the arch of vibration.
		Tent	Air	Tent	Air	
	8	25	24	54	52	1° 35'
		At 8 ^h $\frac{1}{2}$ P. M. in the				{ Tent 33- Air 32
	9	32	32	42	41	
	10	41	41	34	35	
	11	25	25	40	38	1 40 The pendulum swings as before.
	12	30	29	38	41	
	13	31	31	32	33	
	14	28	24			
	15	26	27	At 4 ^h P. M. in the		{ Tent 34 Air 33
	16	18	10	39	48	
	17	25	17	28	28	
	19			39	44	
♀	20	near noon { tent 46 air 55		48	59	
○	22	14	12			
h	28			69		1° 40' { Pend. vibrates about 8' farther on the E. side of O, than on the W. side of O, as before.
March	1			56		
	2			46		
	3			57		
	4			49		
	5			51		
	6			51		
	7			48		
	8			56		
	9			51		
	10			50		
	12	at 11 ^h 11		26		
	13	ditto 7		28		
	14			36		
	15			47		
	16			71		
	17			67		
	28	snow				
	4			91		
	5			95		
	6			95		

The point of the pendulum
swings something farther back
from the arch (shewing the de-
grees and minutes) than it did
when it was set up.

Took down and packed up the
clock.

1767
June

	Height of the ther. at about 7 ^h in the mor. in the Tent Air	Height of the ther. at about 2 ^h in the aft. in the Tent Air
--	--	--

7		93
---	--	----

8		91
---	--	----

9		80
---	--	----

{ The air much altered, being very cool and pleasant.

10 At 4^h $\frac{1}{2}$ P.M. 90 at 7^h P.M. 80

N. B. The thermometer is in the shade, and in the same place it was in last winter.

XLIV. *Extract of a Letter from Rome, to M. Maty, M.D. Sec. R. S. on the extraordinary Heats observed there this last Summer.*

Rome, August 27, 1768.

Read December 15,
1768.

AS I remember, when Mr. M— was here, he seemed desirous to ascertain the degrees of heat and cold; I cannot help mentioning the excessive heat of this summer, which is much greater than has been known in Rome for many years. Friday, the 19th instant, the mercury in a well-regulated thermometer according to Fahrenheit's scale, exposed at a North window, where there was no sun and very little reflection, stood from ten o'clock in the morning until about five in the evening at ninety-nine. About half an hour after sunset it fell to ninety, and at midnight was fallen to eighty-five, where it remained all night. This is the hottest day we have had, but for these three weeks past at midday the mercury has been always above ninety four, and at midnight seldom under eighty-three, which is the more extraordinary as I do not remember to have observed any other summer above eighty-nine at midday, nor above seventy-five at midnight. Notwithstanding this great heat, there was never a more healthy summer at Rome; all the hospitals are almost empty.

James Byres.

XLV. *An*

Received November 21, 1768.

XLV. *An easy Method of making a Phosphorus, that will imbibe and emit Light, like the Bolognian Stone; with Experiments and Observations; by John Canton, M. A. and F. R. S.*

To make the PHOSPHORUS.

Read December 22,
1768.

CALCINE some common oyster shells, by keeping them in a good coal fire for half an hour; let the purest part of the calx be pulverized, and sifted; mix with three parts of this powder one part of the flowers of sulphur; let this mixture be rammed into a crucible of about an inch and a half in depth, till it be almost full; and let it be placed in the middle of the fire, where it must be kept red hot for one hour at least, and then set by to cool: when cold, turn it out of the crucible, and cutting, or breaking it to pieces, scrape off, upon trial, the brightest parts; which, if good phosphorus, will be a white powder; and may be preserved by keeping it in a dry phial with a ground stopple.

The quantity of light a little of this phosphorus gives, when first brought into a dark room, after it has been exposed for a few seconds, on the outside of a window to the common light of the day, is sufficient to discover the time by a watch, if the eyes

have been shut, or in the dark, for two or three minutes before.

By this phosphorus celestial objects may be very well represented; as Saturn and his ring, the phases of the Moon, &c. if the figures of them, made of wood, be wetted with the white of an egg, and then covered with the phosphorus. And these figures appear to be as strongly illuminated in the night, by the flash from a near discharge of an electrified bottle, as by the light of the day.

EXPERIMENT I.

Having put some of the same parcel of the phosphorus into two glass balls, and sealed them hermetically; I placed one of them on the outside of a window facing the South, that it might be very much exposed to the direct rays of the Sun, where it remained from the 25th of December 1764, to the 25th of December 1765. The other was kept during the same time in darkness. After this, they were both exposed to the light, and carried into a dark room together; where the phosphorus in each appeared equally bright.

EXPERIMENT II.

Some of the phosphorus finely powdered, being put into a glass ball, with as much water as would make it adhere to the glass, so as to cover the inside of the ball, which was hermetically sealed, was found gradually to lose its property of imbibing a light, but faster in summer than in winter, so that

at

at the end of the first year, it could not, in the least, be perceived to shine, when taken from the strongest day-light, and carried into a dark room. It was also observed to lose its whiteness by degrees, and to become of a very dark colour, especially on that side of it next to the glass. Some of the phosphorus which was made to stick to the inside of a glass ball hermetically sealed, by means of common spirit of wine, was found after one year to be a little impaired; but some made to stick by means of an ætherial spirit, was found not to be impaired at all.

According to Doctor Nicholas Lemery (in his Course of chymistry, eleventh edition) the exposing the Bolognian stone to the Sun wears it out. But by the first experiment it appears, that a phosphorus of the same kind was not hurt by the Sun in twelve months. Water, indeed, in the second experiment, was found in that time to destroy it. Therefore it is probable, that what the Doctor imputed to the light of the Sun, was caused by the moisture of the air.

EXPERIMENT III.

I mixed a small quantity of the phosphorus with a considerable quantity of spirit of wine in one glass ball, and with æther in another, and sealed them hermetically. When the balls were shook, each of the fluids appeared like milk; but the phosphorus would soon subside when the balls were at rest, and leave the spirit of wine and æther quite clear. After some months, the spirit of wine was found to be tinged with yellow; but the æther, to this time, remains unaltered. By shaking the balls while they are exposed

to the light, the whole of the fluid in each, will appear luminous when carried into a dark room. The æther gives as much light now, as it did at first; but the spirit of wine a little less.

EXPERIMENT IV.

I exposed the dry phosphorus, in one of the glass balls mentioned in the first experiment, to the light of the day, by holding it on the outside of a North window about half a minute; after which it was kept in darkness for two days and a half, and was then found to shine, by putting the glass ball that contained it into a basin of boiling water. On the morrow it was exposed to the light again; and after it had been kept four days and a half in the dark, it gave light when put into boiling water, though not so much as before. In summer, I find, it will not give any light by the heat of boiling water after keeping it fifteen days; but in winter, it will afford a little, after keeping it a month.

EXPERIMENT V.

The phosphorus in each of the two glass balls mentioned in the first experiment was illuminated at the same time and to the same degree, and carried into a dark room. One of the balls was immediately put into a basin of boiling water, and thereupon the phosphorus in it became much brighter than that in the other, and continued so for a short time, but parted with its light so fast, that in less than ten minutes it was quite dark. The other phosphorus still gave a considerable degree

degree of light, and remained visible for more than two hours after, when even the heat of the hand would plainly increase its light.

Bolognian phosphorus is said, by Lemery, and also by Mussichenbroek *, to imbibe less light when hot than when cold, as it appears less bright when carried into a dark room. But this appearance may be caused by its parting with the light it has received faster when in the former state, than when in the latter, according to the last experiment; as it must lose more when hot, than when cold, during the time of conveying it from the place where it takes the light, to a place dark enough to observe it in. And this seems to be the cause also, why Bolognian phosphorus never appears so bright after it has been illuminated, and consequently in some measure heated, by the direct beams of the Sun, as after it has only been exposed, in the shaded open air, to the common light of the day.

EXPERIMENT VI.

The balls used in the last experiment were kept in the dark for two days after, and then each at the same time was put into a basin of boiling water in a dark room: that which had parted with its light in the hot water before, was not visible; but the other appeared luminous for a considerable time.

When the phosphorus has once lost as much of the light it had received, as the heat of boiling water will

* See his *Introductio ad Philosophiam Naturalem*, § 1697. See also § 1704 and 1686.

cause it to part with, it has never after been found, if kept in darkness, to give any more light by that degree of heat. But if it be exposed again to the common light of the day, the experiments may be repeated with the same success as before. This has frequently been done, with some dry phosphorus in glass balls which have been hermetically sealed about four years, without the least injury to the phosphorus; as it appears to be as good now as it was at first.

EXPERIMENT VII.

Let one end of a bar of iron of about an inch square, or a poker, be made red hot, and laid horizontally in a darkened room, till by cooling it ceases to shine, or is but barely visible. Then bring a little dry phosphorus, which has been exposed to light in a glass ball hermetically sealed, as near the hot iron as possible, by holding the ball in contact with it; and the phosphorus, though invisible before, will in a few seconds begin to shine; and will discharge its light so very fast as to be entirely exhausted of it in less than a minute; and then will shine no more by the same treatment, till after it has been exposed to light again. By this heat, light received from a candle, or even from the Moon, may be seen several days after. And phosphorus that will afford no more light by the heat of boiling water, will shine again by the heat of the iron. By this heat also, phosphorus which had been kept in darkness more than six months, was found to give a considerable degree of light.

It was the opinion of the great Sir Isaac Newton, that the rays of light are very small bodies emitted from

from shining substances, and not motion propagated through a fluid medium; for several reasons which he has given in his *Opticks*. Notwithstanding which, it has been urged since his time, that light is nothing but a repellent fluid put into very violent vibrations. Now it appears impossible, to me, at least, if light be nothing but motion propagated through a fluid medium, and not particles emitted from the luminous body, to account for the phenomena in the fifth, sixth, and seventh experiments. That a substance should either give light or not, when its parts are agitated by the same degree of heat, according as it has, or has not been exposed to light, for a few seconds of time, more than six months before; seems plainly to indicate a strong attraction between that substance and the particles of light; by which it keeps many of them, in the common heat of the air, a long time, if not always: for the light the phosphorus gives by being heated to a certain degree appears to be caused by its throwing off adventitious particles, and not by any of its own; since its light will decrease and be entirely gone, before the phosphorus will be hot enough to shine of itself, or to emit particles of light from its own body.

A writer against the Newtonian doctrine of light is pressed with a great difficulty, and asks, if it be possible that a particle can move so far as from the Sun to the Earth, and not frequently impinge upon other particles, when, he says, every part of space must contain thousands of them? But this difficulty will nearly vanish, if a very small portion of time be allowed, between the emission of every particle and the

the next following in the same direction. Suppose, for instance, a lucid point of the Sun's surface to emit 150 particles in one second, which are more than sufficient to give continual light to the eye, without the least appearance of intermission; and then the particles, on account of their great velocity, will be behind one another more than 1000 miles, and leave room enough for others to pass, in all directions.

XLVI. *Astronomical Observations made at Swetzingen, in the Years 1767 and 1768; extracted from several Letters written to Charles Morton, M. D. Sec. R. S. and one to the late Earl of Morton. By Father Christian Mayer, F. R. S. Astronomer to the Elector Palatine.*

Illustrissimo ac celeberrimo Viro ac Domino D. Carolo Morton, Societatis Regiæ Londinensis Secretario, ac ejusdem Synedro, et Academiarum Patro- politanæ et Cæsareo-Leopoldinæ Socio, &c.

S. P. D.

Christianus Mayer S. J. S^{mi} Elect. Pal. Astron. ejusdemque Societatis Regiæ Lond. et Instit. Bononiensis Socius.

Read Dec. 22,
1768.

QUAM tibi mitto observationem eclipsis lunaris, die 3 Jan. hujus anni, in specula electorali Schwetzingensi a me factam, jam velim ita accipias, ut si qui sint defectus in ea commissi, eos non tam mihi quam tempori parum faventi tribuendos putes: paucas enim, quas vides, positiones micrometri, e nubibus fere piscari oportuit, vix semel luna nec nisi prope finem meliori luce resurgente, ob quam causam maculæ nullius immer- certo notare potui. Nihilominus

ominus quantitatem obscurationis, atque ejus initium ac finem bene a me observata cenſeo, neque cum hanc observationem a notis Gallorum et Italorum ephemeridibus tantum diſtare video, ubi potiſſimum hujus erroris fontem inquiram, num in latitudine lunæ an in ejus longitudine, ſatis apud me conſtitutum habeo: ſuſpicio tamen, eum a latitudine potiſſimum repetendum eſſe; ſiquidem ſupputatione ex iſdem Gallorum tabulis a me facta ac diminuta $30''$ latitudinis lunæ, quam cl. De la Lande pro tempore oppoſitionis invenerat = $44'. 42''$ quantitatem obſcurationis obſervationi omnino conformem reperio, non ita tempus veræ oppoſitionis, quod tum adhuc, mutatis varie elementis a calculo ejuſdem De la Lande uno alterove minuto diſſidere deprehendi. Obſervationi huic adjeci aliquot culminationes fixarum ac planetarum, ſolſtitium hybernum proxime circumſtantes, quas ſcio aſtronomis haud ingratas eſſe ob multiplicem uſum comparationum ad perfectionem tabularum tendentium, in quibus equidem occurrent nonnulla, quæ aſtronomis Angliſ. maximæ præciſioni aſſuetis minus ſatisfaciant; verum vel ex eo, illuſtriſſime Morton, facile ipſe intelliges, tecumque intelligent univerſi periti rerum arbitri, quid mihi deſit, quamque difficile ſit, uno quadrante Pariſino mobili $2\frac{1}{2}$ ped. in radio multo plura præſtare. Quis enim, cum per diem ejuſmodi quadrante utitur ad capiendas ſolis altitudines reſpondentes, ingruente vespere idem inſtrumentum ita in plano meridiani ſemper collocet, ut non pluribus ſecundis ab eo ad ortum vel occaſum declinet? Quis culminatione unius ſideris labore altitudinum reſpondentium exacte determinata non in eandem difficultatem recidat; cum reliquorum ſiderum

derum ascensiones rectas a vero situ ejusdem quadrantis pendere nihilominus experiatur? Equidem utor in eum finem meridiano filari ad debitam positionem plani quadrantis percommodo, sed contingit non raro, ut crescentē frigore aut calore nulla alia causa saltem mihi manifesto apparente, quas bene cœpi observationes, exigua deviatio fili penduli turbet, dubiasque proin reddat altitudines siderum apparentes. Huic quoque malo promptam medelam afferre licet exigua elevatione aut depressione cochlearum quadrantem sustinentium, verum non sine manifesto periculo destruendi planum verticale. Taceo reliqua instrumenti hujus vitia: hæ certe difficultates sunt tantæ, ut iisdem colluctari diutius aut non possim aut nolim, quibusque nemo citius meliusque liberare me potest quam Anglus artifex.

Datum Heidelbergæ, die
15 Jan. 1768.

Observationes cœlestes factæ Schwetzingæ in Specula
Arcis Electoralis, anno 1767 et 1768.

TABULA MERIDIEI VERI.

Altitudo limbi solis
apparenter superioris
secundum purgatam a re-
fractione et parallaxi.

Dies anni 1767 et 1768.
Meridies verus et correctus.

Dec.	22	12	16	51,5*	16	55	46
	23	12	17	23,6*	16	56	20
Jan.	1	12	24	7,0			
	2	12	24	46,5*			
	3	12	25	37,0			
	4	12	26	8,5			
	5	12	26	51,0			
	6	12	27	34,5*	17	51	35
	7	12	28	55,5			

Meridies asterisco * no-
tati indicant tempus
verum meridiei il-
lius diei exacte per
altitudines solis cor-
respondentes deter-
minatos esse.

Notandum, die 24 Dec. horologium pendulum duobus circit. min. quietum confitisse; ob eamque causam meridiem diei 1 Jan. nequam consentire cum meridie dierum 22 et 23 Dec. habita nempe ratione ordinatæ accelerationis; a die vero 1 Jan. usque ad 6 tum incrementum diurnum accelerationis penduli continuo augeri fere in progressionem naturali una cum gradu frigoris per illos dies incrementante, ut adjecta Tabula docet.

T A B U L A.

Maximus grad. frigoris in Therm.
Reaumuriano.

Quantitas accelerationis
penduli supra motum
medium Solis.

Jan. die 1	12,5 infra 0	0 11,3
2	13,0	12,5
3	15,5	14,1
4	16,0	15,5
5	18,5	16,9

Altitudines quædam fixarum tempore vero culminationis proxime observatæ Schwetzingæ Quadrante
 $2\frac{1}{2}$ ped.

1767. Temp. verum. Altitudines refractione non purgatæ.

Dec.	22	5	52	47,2	63	26	47	Capit. Antromedæ, α 2.
		5	57	44,0	54	32	34	Extrema in ala Pegasi Algen γ 2.
		7	36	58,0	58	47	43	In aure Arietis, γ 4.
		11	56	19,5	63	11	34	Pes Castoris, η 4.
		11	57	37,0	63	5	35	♄ Saturnus in media filorum intersectione medius.
		12	4	20,0	63	14	35	In pede Pollucis, μ 4.
		12	19	41,6	57	13	0	Lucida in tibia Pollucis, γ 3.
		12	30	17,0	24	14	31	Syrus.
		12	45	37,5	61	31	40	In genu Castoris, ζ 1.
		13	1	29,2	63	1	45	In femore Pollucis, δ 3.
		17	26	52,2	17	14	0	In rostr. Corvi, α 1.
		17	28	14,5	21	23	59	In ala præcedente Corvi, γ 3.
		18	14	25,3	25	44	22	Sidus rææ magnitud. intense rubrum ob limbum fatis bene terminatum cometa simile.
		19	3	59,2	30	12	15	Spica Virginis.
		19	8	59,2	34	4	55	Limbus apparenter superior 21 Jovis.

Continuatio

Continuatio præcedentium Observationum.

	h	'	"	o	'	"	
Dec. 23	12	17	23,6				Meridies verus.
	5	52	56,9	54	32	43	Algen, γ 2
	7	32	21,0	58	47	20	In aure Arietis, γ 4.
	11	52	33,5	63	5	48	Centrum Saturni $\frac{1}{2}$.
	11	59	40,0	63	14	55	In pede Pollucis, μ 3.
	12	15	3,0	57	12	55	Lucida in tibia Pollucis, γ 3.
	13	30	18,0	16	22	40	In summitate Argonavis, ζ 4.
							Comitibus duabus stellulis.
	13	47	56,0	17	1	15	In puppi Argonavis, ρ 3.
1768.							
Jan. 2	11	4	38,5	63	6	55	Centrum Saturni.
	11	7	14,5	63	11	22	Pes Castoris.
	11	41	5,6	24	14	78	Syrius.

Culminationes Centri Lunæ Schwetzingæ observatæ,
1768.

Mora Transitus Lunæ per merid.	die 2 Jan.	2	26
	3 Jan.	2	24,7

Altitudo limbi apparent. superior.

Jan.	2	10	52	21,1	65	37	0
	3	11	48	42,6	64	0	10
	6	14	16	27,2	52	8	20
Eodem	20	40	13,3	24	10	3	Centrum Veneris γ .

Hæ altitudines eo sensu apparentes sunt, quod ab errore instrumenti, ut cæterorum fiderum omnium altitudines, correctæ sunt, non item a refractione et parallaxi, aberratione, &c. Addenda quoque est semidiameter lunæ pro habenda vera altitudine centri.

Barometrum die 22 Dec. in meridie fuit 27 dig. 11 $\frac{1}{2}$ lin. vespere eodem die 28 dig. Die 23 = 28 dig. 1 lin.

Die 2 Januarii = 28 dig. 2 lin.
28 — 25
27 — 11

Therm. Reaumurianam die 22 Dec. mane fuit 2 infra 0, vespere 4. Die 23 mane 6. vespere 7. circa meridiem 5. infra terminum glaciæ: de reliquis diebus constat ex superiore Tabula.

Notandum,

Notandum, culminationes diei 22 Decemb. supra recensitas propiores vero esse, quam illas diei 23: siquidem factio calculo inveni caput Andromedæ α 2, die 22 Dec. culminare debuisse hora 5, 53' 13" temp. ver. id est 26" tardius quam ex observatione habui 5^h 52' 47": veram autem culminationem ipsius *Algen* die 23 Dec. accidisse 5^h 53' 39" \pm T v. qualem observaveram 5^h 52' 56", 9. Unde patet aberrationem plani quadrantis ad ortum pro die 22 Dec. fuisse circit. 26 secundorum; pro die vero 23 Dec. eam aberrationem fere duplo fuisse majorem ad ortum: dixi fere; quia hæc ipsa tempora vera culminationum, ut sint exactissima, aberrationis et nutationis correctiunculis emendanda forent.

Observationes duarum Immerfionum primi Satellitis Jovis, factæ Schwetzingæ, tubo Dollondi, a Christiano Mayer, S. J.

	Temp. ver.
Anno 1767, die 30 Dec. Satelles I. Jovis videri definit	16 58 59
Anno 1768, die 6 Jan. Satelles I. adhuc apparet	18 48 51
Disparuit	18 49 4

Observatio posterior melior est prima, ob cælum minus vaporosum.

Oppositio Lunæ ecliptica die 3tio Januarii, in Specula Electorali Schwetzingensi observata, a P. Christiano Mayer.

Tubo 6 pedum micrometrum habente, tempore nubilo.

Temp. ver.			Partes Lunæ obcuratæ.	Digitum obcurati.
15 44 29	Penumbra incipit ad partes Lybiæ et Arab.			
46 9	Eadem fit densior			
48 55	Videtur eclipsis esse initium		686	2 40
16 8 30			990	3 51
15 52			1334	5 11
20 54			1400	5 26
32 6			1412	5 28
36 6			1458	5 40,9
47 48	Maxima obscuratio observata			

EMERSIONES.

17 39 35			1255	4 53
				Temp

Temp. ver.						Partes	Dig.	
h	'	"				Lunæ	obf.	
						obscur.		
17	33	32	-	-	-	1166	4	32
	37	10	-	-	-	976	3	48
	41	24	-	-	-	807	3	9
	47	34	-	-	-	726	2	50
	50	19	-	-	-	672	2	38
	51	41	-	-	-	576	2	16
	57	52	-	-	-	474	1	52,3
18	1	38	-	-	-	299	1	11,6
18	4	44	Finis dubius					
	6	20	Finis certus tubo 6 pedum					
	6	53	Idem finis tubo Dollondi ab alio observatus					
			Diameter lunæ toto tempore observa-					
			tionis assumpta in partibus circuli					
			in partibus micrometri					

Porro ex hac observatione patet durationem totius eclipsis fuisse $2^h 17' 25''$ eamque optime consentire tabulis Gallicis et Italicis; medium autem eclipsis ex observatione inventum fuisse $= 4^h 57' 40''$, quod ex tabulis De la Caille debebat apud nos esse $5^h 3' 21''$; ex calculo De la Lande $5^h 1' 1''$; ex ephemeridibus Bononiens. $5^h 2' 28''$; quantitatem obscurationis a me ex observatione inventam 5 dig. 41', quæ ab astronomis Parisinis supputata est 4 dig. 52' a Bononiensibus 4 dig. 53'.

Immerfiones Satellit. Jovis Swetzingæ observatæ, Mensi Martio, 1768.

			Temp. ver.		
			h	'	"
Martii	8	Satelles I. 24 obscure adhuc apparet	-	-	-
		Idem omnino disparet	-	-	-
			17	17	0
			17	17	18
	9	Satelles II. obscurius adhuc apparet	-	-	-
		Idem omnino disparet	-	-	-
			14	16	17
			14	16	37
	9	Satelles III. obscurius adhuc apparet	-	-	-
		Idem omnino disparet	-	-	-
			14	21	18
			14	22	6
	10	Satelles I. adhuc obscure apparet	-	-	-
		dubie apparet	-	-	-
		omnino disparet	-	-	-
			11	46	55
			11	47	0
			11	47	15

		Temp. ver.		
		h	'	"
Martii 24	Satelles I. luce imminuta limbum Jovis	15	38	25
	tangere videtur			
	Idem omnino disparet			
	Idem omnino disparet			
26	Satelles I. obscurius adhuc apparet	10	7	35
	omnino disparet	10	7	45
27	Satelles II. obscurius adhuc apparet	8	43	45
	Idem omnino disparet	8	44	6
Aprilis 11	Satelles I. prope limbum Australem Jovis	10	41	26
	emerfisse videtur			

Omnes hæ observationes noto tibi tubo Dollondi Schwetzingæ factæ sunt, cælo præter morem puro, statu penduli et pridie et ipso plerumque observationis die per altitudines solis respondententes penitus explorato, atque inde deducto tempore medio ob variationem declinationis solis correctæ, fasciis Jovis semper apparentibus, et, ut æstimare poteram, sub angulo sex vel septem graduum ad axem majorem Jovis inclinatis ab ortu in occasum, alter enim axis minor fere directionem verticalis a Borea ad Austrum plus minus pro varia Jovis altitudine constanter sequi videtur, quam quidem compressionem figuræ Jovialis evidenter observavi, nunquam tamen melius, quam die 26 Martii, quo prope marginem Jovis australem paulo ante immerfionem, maculam præterea rotundam situ inter ortum et occasum fere mediam in parte, ut dixi, disci Jovialis australi clare vidi.

Conferendo Immerſiones ſupra dictas cum ephemeridibus De la Lande maxima differentia oritur, ex illa 10 Martii: nempe,

	r	''
Minima differentia ex 8 Mart. - - - - -	25	12
	24	12
Differentia eſt - - - - -	1	
Semi-differentia eſt - - - - -	0	30
Differentia meridianorum - - - - -	24	42
Sed habita ratione immerſionis diei 11 April. differentia noſtrorum meridianorum Pariſis inter et Schwetzingam ex facta cum ephemeridibus collatione oritur - - - - -	26	30
Differentia immerſionum et emerſionum - - - - -	1	48
Cujus dimidium - - - - -	0	54
Additum priori determinationi - - - - -	21	42
Producit differentiam meridianorum - - - - -	25	36

Illuſtriſſimo et excellentiſſimo Domino, Domino Comiti de Morton, Societatis Regiæ Londinenſis Præſidi digniſſimo, &c. &c.

Christianus Mayer S. J. Societatis Regiæ Londinenſis et Inſtit. Bonon. Socius.

Heidelb. Jul. 20, 1768.

SUBJUNGO quod ex nupera eclipſi totali lunæ diei 29 Junii mihi inclementia cœli fecit reliquum. Cœlo omni ſpiſſis nubibus conſtanter obducto, luna non niſi paulo ante totalem ſui immerſionem horiſonti proxima apparuit, quo tempore tres ſequentes poſitiones tubo dioptrico ſexpedili micrometro armato capere potui.

Anno 1768, die 29 Junii.

Tempus verum.				Partes lucidæ lunæ.				Partes micrometri.			
h	'	"			'	"					
15	31	46	-	-	-	0	7	25.7			
15	34	51	-	-	-	0	5	53.4	-	-	575
15	39	34	-	-	-		3	31.5	-	-	343

Ex his assumpta diametro horizontali lunæ 31' 4", et motu horario lunæ a sole 30' 11", inveni initium hujus eclipsis ad meridianum Schwetzingensem proxime accidisse

Totalem immersionem Lunæ - - - - - 15 51. 23

Et reductione facta ad meridianum Londinen- } h ' " pro initio
sem speculæ domestice cl. D. Short } 14 3 33

Et pro immersione totali - - - - - 15 15. 44

Quod si aliqua ratione observationibus illinc factis consentiat in tanta aëris nostri malignitate, fortunæ tribuo.

Emerfiones aliquot Satellitum Jovis observatæ Schwetzingæ tubo Dollondi 10 ped. anno 1768.

	Temp. ver.			
	h	'	"	
Die 27 Maii cœlo sereno				
Satelles III. omnino mihi ex oculis evanescit seu immergitur.	10	27	26	
Eadem nocte Satelles IV. observante sereno principe Wil-				
helmo de Gelbapfen, emergit videtur	11	9	44	
Die 3 Junii emerfio I ^{mi} Satellitis a me observata inter nubes	13	5	5	
Die 5 Julii emerfio I ^{mi} Satellitis cœlo sereno ab alio, me				
absente, observata	9	33	42	

Hæc ultima observatio ab homine juvene exercitata facta, cœlo sereno, longe præferenda est illi die 3 Junii a me factæ, Jove fere inter atras nubes per vices, incedente.

Christianus Mayer, S. J.

XLVII. *Observations of the Transit of Venus over the Sun, and the Eclipse of the Sun, on June 3, 1769; made at the Royal Observatory. By the Rev. Nevil Maskelyne, B. D. F. R. S. and Astronomer Royal.*

Read June 15, 1769. **T**HE weather, which had been cloudy or rainy here, with a south wind, for the greatest part of the day, began to clear up at 4 o'clock in the afternoon, the wind having returned to the west, the same quarter in which it had been the afternoon before, which was remarkably fine and serene, though it changed early in the morning preceding the transit. Towards the approach of Venus's ingress on the Sun, the sky was become again very serene, and so continued all the evening, which afforded as favourable an observation of the transit here as could well be expected, considering that the Sun was only $7^{\circ} 3'$ high at the external, and $4^{\circ} 33'$ at the internal contact. I observed the external contact of Venus at $7^{\text{h}} 10' 58''$ apparent time, with an uncertainty seemingly not exceeding $5''$, and the internal contact, by which I mean the completion of the thread of light between the circumferences of the Sun and Venus, at $7^{\text{h}} 29' 23''$ apparent time, with a seeming uncertainty of only $3''$, for so long was the thread of light in forming, or the Sun's light in flowing round and filling up that part of his circumference which was obscured by

Venus's exterior limb. Nevertheless, I would not hence infer, that observations made by astronomers in distant places should agree together within such narrow limits; for I know they will not even in the same place, and that a difference in the skill or judgment of the observers, in the telescopes, and perhaps in some other little circumstances, not easily distinguished, may produce much greater disagreements, especially if the Sun be low, as it was here; in like manner as in observing the eclipses of Jupiter's satellites, the immersion or emersion shall often seem instantaneous, or nearly so, equally to two observers in distant places, and yet the absolute times of the observations may differ a minute of time or more from each other, owing to the difference of telescopes, weather, or other circumstances. Indeed, in the present case, the limit of differences is certainly much narrower; but what it is I shall not at present venture to suggest, as that may better be done when all the observations that shall have been made of the transit are collected together. The telescope which I used, was an excellent reflecting one of two feet focus, made by the late ingenious Mr. Short, and is the same with which the last transit was observed here by Mr. Charles Green. I applied the magnifying power of 140 times, and used smoked glasses to defend the sight, which are much preferable to black or red glasses, as shewing the objects more distinct, and being much more pleasant to the eye.

I shall now endeavour to describe, as accurately as I can, some other phenomena which I noted during the immersion of Venus, and to mention some others, which by some ingenious persons were expected to have

have been seen, but which I could not discover.

It had been thought by some, that Venus's circumference might probably be seen, in part at least, before she entered at all upon the Sun, by means of the illumination of her atmosphere by the Sun; I therefore looked out diligently for such an appearance, but could see no such thing.

I was also attentive to see if any penumbra or dusky shade preceded Venus's first impression on the Sun at the external contact, such a phenomenon having been observed by the Rev. Mr. Hirst, F. R. S. at the former transit of Venus, in 1761, which he observed with much care and diligence at Madras, in the East-Indies; but I could not discern the least appearance of that kind. I would not, however, be therefore thought to call in question either Mr. Hirst's discernment or fidelity; as I am sensible that the tremors of the limbs of the Sun and Venus, occasioned by the vapours at the altitude of 7° , might easily obscure a faint object.

When Venus was a little more than half immersed into the Sun's disc, I saw her whole circumference completed, by means of a vivid, but narrow and ill-defined border of light, which illuminated that part of her circumference which was off the Sun, and would otherwise have been invisible. This I might, probably, have seen sooner, if I had attended to it. I continued to see it till within a few minutes of the internal contact, and grew apprehensive that it would prevent the appearance of the thread of light, when it came to be formed, but it disappeared about two or three minutes before, as well

well as I can remember: after which the regularity of Venus's circular figure was disturbed towards the place where the internal contact should happen, by the addition of a protuberance, dark like Venus, and projecting outwards, which occupied a space upon the Sun's circumference, which bore a considerable proportion to the diameter of Venus. Fifty-two seconds before the thread of light was formed, Venus's regular circumference, supposed to be continued as it would have been without the protuberance, seemed to be in contact with the Sun's circumference supposed also completed. Accordingly, from this time, Venus's regular circumference, supposed defined in the manner just described, appeared wholly within the Sun's circumference; and it seemed, therefore, wonderful that the thread of light should be so long before it appeared, the protuberance appearing in its stead.

At length, when a considerable part of the Sun's circumference, equal to $\frac{1}{4}$ or $\frac{1}{2}$ th of the diameter of Venus, remained still obscured by the protuberance, a fine stream of light flowed gently round it from each side, and completed the same in the space of three seconds of time, from $7^h 29' 20''$ to $7^h 29' 23''$ apparent time; and Venus appeared wholly within the Sun's lucid circumference; but the protuberance, tho' diminished, was not taken away till about $20''$ more, when, after being gradually reduced, it disappeared, and Venus's circular figure was restored.

An ingenious gentleman of my acquaintance having desired me to examine if there was any protuberance of the Sun's circumference about the point of the internal contact, as he supposed such an appearance,

pearance ought to arise from the refraction of the Sun's rays through Venus's atmosphere, if she had one; I carefully looked out for such a circumstance, but could see no such thing; neither could I see any ring of light round Venus, a little after she was got wholly within the Sun: but, I confess, I did not re-examine this latter point afterwards, when she was further advanced upon the Sun, at which time other persons at the observatory saw such an appearance.

How far the ring of light, which I saw round that part of Venus's circumference which was off the Sun, during the immersion, may deserve to be considered as an indication of an atmosphere about Venus, I shall not at present inquire; but I think it very probable, that the protuberance, which disturbed Venus's circular figure at the internal contact, was owing to the enlargement of the diameter of the Sun, and the contraction of that of Venus, produced by the irregular refraction of the rays of light through our atmosphere, and the consequent undulation of the limbs of the two planets; the altitude of Venus being only $4^{\circ} 48'$, though the Sun's limb was more distinct and steady than usual at that altitude. This conjecture seems corroborated by two circumstances: one is, that Venus's limb, from its first appearance to the total immersion, as well as afterwards, was very ill defined, and undulated very much; the other is, that her horizontal diameter, which I measured soon after the internal contact with an excellent achromatic object-glass micrometer, fitted to the two-feet reflecting telescope, was only $55\frac{3}{4}''$, by a mean of eight trials, or about $3''$ less than it should have been, from the observations made, with the like instrument, at the transit

transit of Venus in 1761, by Mr. Short, Mr. Canton, Mr. Haydon, and Mr. Maſon, when the Sun was at a conſiderable altitude; and moſt likely the Sun's diameter was enlarged in proportion, though it might have been difficult to have aſcertained it by actual meaſure, had time allowed me to make the experiment with the ſame micrometer before the Sun entered into a black cloud near the horizon.

Six other perſons alſo obſerved the contacts of Venus here, and noted ſome other phænomena. Their names are, the Rev. Malachy Hitchins, a gentleman well acquainted with aſtronomy and aſtronomical calculations, who has made and examined many belonging to the Nautical Almanac, and has been ſo obliging as to come here and aſſiſt me in making aſtronomical obſervations, during the abſence of my aſſiſtant, Mr. William Bayley, who is gone to the North Cape, by appointment of the Royal Society, to obſerve the tranſit of Venus there. The others are, the Rev. William Hirt, who obſerved the former tranſit of Venus, in 1761, at Madraſ; John Horſley, Eſq; a gentleman whom I had the pleaſure of firſt commencing an acquaintance with during my voyage from St. Helena to England, in the Warwick Eaſt-India ſhip, and who then, and in ſeveral voyages ſince to the Eaſt-Indies and home again, obſerved and calculated the longitude from diſtances of the Moon from the Sun and fixed ſtars with the greateſt accuracy. Mr. Samuel Dunn, who has had a good deal of practice in making aſtronomical obſervations, and who carefully obſerved the former tranſit of Venus, in 1761, at Chelſea; Mr. Peter Dollond, whoſe great ſkill in conſtructing achromatic and reflecting teleſcopes;

lescopes; and Mr. Edward Nairne, whose skill likewise in the same way, and in making all kinds of mathematical and philosophical instruments are sufficiently known to the public.

Mr. Horsley and Mr. Dunn observed with me in the great room; Mr. Hitchins and Mr. Hirst in the eastern summer-house; and Mr. Dollond and Mr. Nairne in the western summer-house; by three clocks placed in the respective rooms, which were compared with the clock in the transit room, before the external contact, and again after the internal contact was past; whence the times of the observations, as noted by the clocks, were reduced to the time of the transit clock, and thence to apparent time.

Their observations, together with my own, are given in the following table, as reduced to apparent time.

	External contact.			Regular circumference in contact.			Thread of light completed, or the internal contact.			Telescope made use of.	Magnifying power.
	h	'	"	h	'	"	h	'	"		
N. Maskelyne	7	10	58	7	28	31	7	29	23	2 feet reflector.	140
M. Hitchins	7	10	54	7	28	47	7	28	57	6 f. reflector.	90
W. Hirst	7	11	11	—	—	—	7	29	18	2 f. reflector.	55
J. Horsley	7	10	44	7	28	15	7	29	28	10 f. achromatic.	50
S. Dunn	7	10	37	7	29	28	7	29	48	3½ f. achromatic.	140
P. Dollond	7	11	19	—	—	—	7	29	20	3½ f. achromatic.	150
E. Nairne	7	11	30	—	—	—	7	29	20	2 f. reflector.	120

Mr. Dollond and Mr. Nairne used telescopes of their own construction; but they did not wait till the thread

thread of light was formed at the internal contact, but noted the time, when they judged it was just ready to be formed. The $3\frac{1}{2}$ feet achromatic telescopes were those made with 3 object-glasses.

The differences between the different observations seem pretty considerable, and greater than I expected, considering that all the telescopes may be reckoned pretty nearly equal, excepting the 6 feet reflector, which is much superior to them all; and to its greater excellence and distinctness I principally attribute the difference of $26''$ by which Mr. Hitchins saw the internal contact before me; as I can depend upon his observations. Possibly the greatness of the differences might arise from the low altitude of the Sun and Venus; and then the like differences would not be so much to be feared in places where the observation may be made at higher altitudes; otherwise the sun's parallax will not be deducible from the transit of Venus with that accuracy which has been expected.

The other appearances about Venus, noted by the six observers, which they have communicated to me, are as follows:

Mr. Hitchins remarks, that, at the first contact, though there was a tremulous motion in the Sun's limb, yet that part of it which the planet entered was very well defined, and the first impression of Venus appeared to be instantaneous, and as a black, sharp point. At the internal coincidence of circumferences, the fluctuation of the Sun's limb was increased, and the limb of Venus being affected in like manner, there was an uncertainty of about $10''$ in estimating the said coincidence; but at the breaking in of the thread of light between the limbs, there was not

not a greater uncertainty than a second and half of time. At the internal coincidence of circumferences, the limb of Venus next to that of the Sun being protuberant, her vertical diameter appeared to be longer than the horizontal one; but when the Sun approached the horizon, and was scarce above a degree high, Venus's horizontal diameter appeared to be sensibly longer than the vertical, which was, probably, owing to refraction. After the internal contact, there appeared a luminous Ring round the body of Venus, about the thickness of half her semi-diameter; it was brightest towards Venus's body, and gradually diminished in splendor at greater distances, but the whole was excessive white and faint. This radiancy round the planet seemed to him to be greater in Mr. Nairne's 2 feet telescope than in the 6 feet Newtonian reflector.

After the second or internal contact, Mr. Hirst left off observing with Mr. Dunn's 2 feet reflector, and had a sight of Venus in the 6 feet Newtonian reflector, in which he thought he perceived a glimmering of light about the upper part of the circumference of Venus, or that part of the planet which entered last into the Solar disc.

After Venus was got within the Sun's disc, a light a little weaker than that of the Sun, of a purplish colour, appeared to Mr. Horsley, to the left-hand of Venus, which is really to the right, the telescope inverting objects. This light he saw for six or seven minutes.

From $7^h 28' 26''$ to $7^h 28' 30''$ apparent time, Mr. Dunn saw a very faint rim of light at Venus's exterior

exterior limb. After Venus was wholly on the Sun, he saw a faint ring of light surrounding her, both with the $3\frac{1}{2}$ feet telescope, and Mr. Nairne's 2 feet reflector.

When $\frac{1}{3}$ ^d of Venus's diameter was entered upon the Sun, Mr. Dollond first saw a light about the exterior limb of the planet: this light, during all the time of its continuance, appeared rather reddish, and in all respects like irregular refracted light. After Venus was wholly entered upon the Sun, he saw a faint ring surrounding her.

After Venus was wholly entered upon the Sun, and her exterior limb was near one of her semi-diameters distant from the Sun's circumference, Mr. Nairne saw a faint light round the planet, rather brighter and whiter than the body of the Sun.

Fortunately, the weather was as favourable for the observation of the eclipse of the Sun, the next-morning, as it had been the evening before for that of the ingress of Venus upon the Sun; which is of the more consequence, as the comparison of it with the observations which may be made of it in the northern and eastern parts of the world will serve to settle the longitudes of those places, and consequently render the observations which may be made there of the transit more useful and valuable.

I observed the beginning of the eclipse at $18^h 38' 54''$, and the end at $20^h 23' 30''$ apparent time, with the 2 feet reflector, using the magnifying power 90 times. And at $19^h 29' 31''$ apparent time, I observed the greatest eclipse, at which time I found the remaining lucid parts of the Sun $15' 15''$ with Dollond's micrometer, assuming the horizontal diameter of the Sun

31' 31'', whence the value of the scale of the micrometer was determined for the present purpose. Hence the eclipsed parts of the Sun were 16' 16'' or 6^{dig}. 11',62 on the northern part of his disc.

Mr. Hitchins observed the beginning of the eclipse with a $3\frac{1}{2}$ feet achromatic telescope magnifying 150 times (the same with which Mr. Dollond observed the contacts of Venus), at 18^h 38' 59'', and the end of the eclipse with the 6 feet reflector with the magnifying power 90, at 20^h 23' 35'' apparent time. And Mr. Samuel Dunn observed the beginning of the eclipse at 18^h 39' 9'', and the end at 20^h 23' 33'' with the other $3\frac{1}{2}$ feet achromatic telescope, magnifying 140 times, the same with which he observed the contacts of Venus. Several inequalities in the Moon's circumference, seen upon the Sun's disc during the eclipse, were distinctly discerned by all of us, the air being very clear, and the objects steady.

The whole series of measures of the lucid parts, which I took with the achromatic object glass micrometer applied to the 2 feet telescope, was as follows;

Apparent time.			Lucid parts.	
h	'	''	'	''
19	22	13	—	15 40,5
	24	21	—	15 26,5
	26	9	—	15 20,9
	28	26	—	15 15,6
	30	14	—	15 14,5
	31	44	—	15 16,4
	32	30	—	15 16,4
	33	19	—	15 19,8
	34	28	—	15 25,4
	36	19	—	15 35,9
	37	56	—	15 49,1

E R R A T A.

- P. 7. l. 10. *for confusion read concussion.*
 P. 28. l. 8. *for $Q^3 \times Q^2$ read $q^3 \times Q^2$.*
 P. 29. *in the note, for 00379755 read 90379755.*
 P. 30. l. 2. *for hypothesis read hypotheles.*
 Ibid. *throughout the table, for δ read δ .*
 P. 32. l. 16. *for 1759 read 1761.*
 P. 175. l. 15. *for being any number read x being any number.*
 P. 176. l. 4. *for tohat read to that.*
 P. 179. l. 18. *for $\frac{B}{\frac{1}{3}}$ read $\frac{B}{2\frac{1}{3}}$*
 P. 203. l. 24. *for thing thing read thing.*

A N
I N D E X
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